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Cover An antiproton enters a propane bubble chamber. See page 9. [From Agnew *et al.*, *Phy. Rev.* **110**, 994 (1958)]

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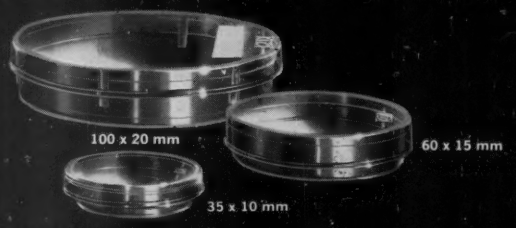

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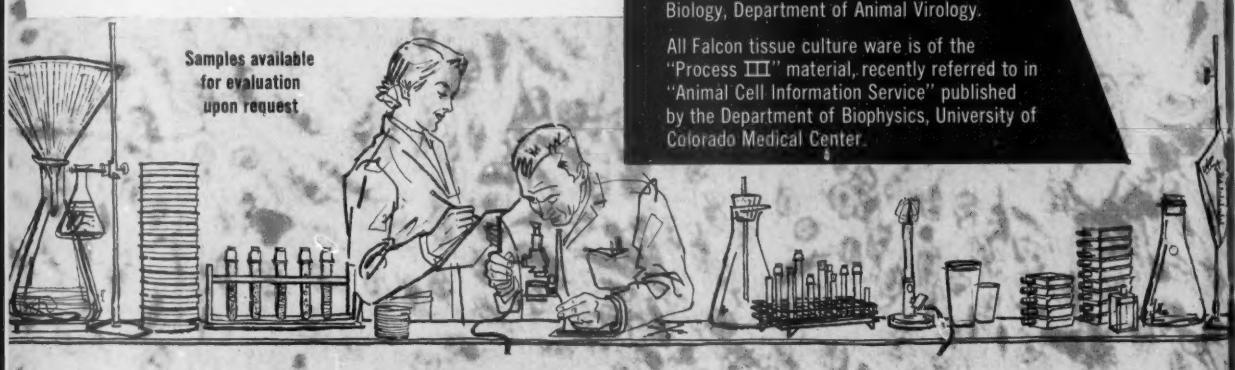

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Humane Treatment of Animals

When Mead and Metraux asked high school students to describe a scientist, the composite image turned out to be that of a pretty unpleasant sort of fellow: "He may wear a beard, may be unshaven and unkempt . . . ." His laboratory is identified by "the bubbling of liquids in test tubes and flasks, the squeaks and squeals of laboratory animals, the muttering voice of the scientist . . . . He experiments with plants and animals, cutting them apart, injecting serum into animals" [*Science* **126**, 384 (1957)].

This unpleasant, even sinister, image seems to have influenced the 13 senatorial sponsors of a bill (S. 3570) aimed at curbing scientists' presumably inhumane treatment of their animal subjects [*Science* **131**, 1658 (1960)]. The sponsors agree that animal experimentation is essential. They agree that experimental animals should be treated humanely. With these premises scientists would also agree. But few will agree with the implicit assumption that cruelty to animals is a common occurrence in research laboratories or with the proposed means of preventing it.

The bill would require recipients of federal grants who engage in experiments or tests upon vertebrates to secure licenses from the Secretary of Health, Education, and Welfare. It would require agencies of the Federal Government and institutions receiving federal grants to comply with a number of regulations concerning the care of laboratory animals. The regulations themselves are unobjectionable; they say what every good laboratory director insists upon anyway. The objectionable features are the procedures required: advance approval of experimental plans by the Department of Health, Education, and Welfare, burdensome record keeping, annual or more frequent reports to HEW, additional costs for the government and for every laboratory involved, and the general nuisance of establishing and living under a new and unnecessary amount of red tape.

It would be silly to deny that there may be occasional violations of good practice, but passage of this bill would punish the many in the hope of preventing lapses by the few. It would hamper the work of many laboratories and especially those that observed its requirements most meticulously.

The bill will not be acted upon this year; but it may be introduced again. In the long run, the solution is not to oppose each nuisance bill as it is introduced, but to correct the false ideas that allow such bills to be taken seriously. We are reminded of the very different image of the scientist drawn by John R. Baker in his essay on *Science and the Planned State* (Allen and Unwin, London, 1945). Among the social duties of a scientist, Baker includes the obligation "to encourage kindness to animals." He contrasts the humane treatment of laboratory animals with the cruelty involved in shooting and trapping wild animals and in the methods of castration and ovariotomy frequently used on domestic animals. Because the biologist knows more than other men about the pain sense of animals, Baker suggests that biologists try to educate others concerning the pain they inflict on animals.

The supporters of S. 3570 seem to have taken their cue from the high school students' image of a scientist instead of from the behavior and principles of scientists themselves.—D.W.

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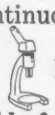

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1 July 1960, Volume 132, Number 3418 **SCIENCE**

Nuclear Properties of Antinucleons

The study of antiprotons reveals other antiparticles previously predicted and also unpredictable aspects of the scattering and annihilation process.

Emilio Segrè

I must begin by thanking the Swedish Academy for the great honor they have bestowed on me. The names of the previous recipients of the Nobel award lend such great prestige to the award, that I feel very humble in joining the company. At the outset I must also mention the names of two people who have had, in different ways, a very great influence upon all my work. Of Enrico Fermi I would only say, quoting Dante as he himself might have done,

> Tu se' lo mio maestro e il mio autore
> Tu se' solo colui da cui io tolsi
> Lo bello stilo che mi ha fatto onore.
>
> (Thou art my master and my author;
> Thou alone art he from whom I took
> The good style that hath done me honor.)

I learned from him not only a good part of the physics I know but, above all, an attitude of constant devotion to science which has affected all my work. Ernest Orlando Lawrence created the instruments with which most of my work was done. Although I belong scientifically to a different tradition and outlook, it was only through the instruments developed at his instigation and under his leadership that most of my own researches became possible. This is especially true for the most recent one: the antiproton.

By 1954 the bevatron had been developed and tested. It had been purposely planned for an energy above the threshold for forming nucleon-antinucleon pairs, and many physicists, including my colleagues and me, naturally thought of means for hunting the elusive antiproton. Although its existence was very probable, a definite experimental proof was lacking, and being aware of the crucial importance of the problem for the extension of Dirac's theory from the electron to the nucleon, we tried to design an experiment which would give a definite answer (*1*). The final apparatus has been described elsewhere (*2*).

Other experiments involving photographic detection were also planned at that time and came to fruition soon after the success of the first experiment (*3*).

The properties used for the identification of the antiproton were predicted by Dirac long ago and were used as a guide in finding the particle. However, once it was found, we faced a host of new problems, and it is to those that I direct this discussion.

The author is professor of physics at the University of California, Berkeley. He shared with Owen Chamberlain the Nobel Prize in physics for 1959. This article is adapted from his Nobel lecture delivered 11 December 1959, in Stockholm, before the Royal Swedish Academy of Sciences.

Systematics of Particles

I will be very brief concerning the experimental developments. Here, great emphasis has been put on the development of better antiproton beams. By "better" I mean beams in which there are more antiprotons per unit time and in which the ratio of the number of antiprotons to unwanted particles is higher. Suffice it to say that now it is possible to have, at Berkeley, beams with about ten antiprotons per minute instead of one every 15 minutes as in 1955, and beams in which antiprotons are about one in ten particles instead of one in 50,000, as in 1955. The improved beams allow us to make more difficult and complicated experiments, and the development of electronics and bubble chambers has kept pace with the increased possibilities. I may add that the complications in which we are entering now are by no means a cause of joy to the experimenters who have to cope with them, and that they are properly considered the heavy price to be paid in order to obtain more detailed physical information.

Some of the problems raised by the very existence of the antiproton have a predictable solution, although the prediction does not derive from anything as solid as Dirac's theory. We could, for instance, predict with complete confidence the existence of the antineutron and of all the antiparticles of the baryons, although it might require considerable skill to find them. In fact, antineutrons are certainly formed copiously at the Bevatron, but the primary antineutrons are very difficult to identify. For this reason, immediately after the discovery of the antiproton it was suggested that the antineutron should be found by investigating the charge-exchange reaction in which a proton and an antiproton give a neutron and an antineutron (*4*). In a very ingenious and elegant counter experiment, Cork, Lambertson, Piccioni, and Wenzel did demonstrate the existence of the antineutron some time ago (*5*). Their method was based on a counter-technique and used the reaction

$$p + \bar{p} \rightarrow n + \bar{n},$$

Table 1. Spin, parity, and I-spin of nucleons and antinucleons.

	Proton	Neutron	Antiproton	Antineutron
Spin, S	½	½	½	½
I-spin, T	½	½	½	½
Third component of I-spin, T_3	½	−½	−½	½
Parity	+	+	−	−

which is called charge exchange because we can interpret it as the passage of the electric charge from the proton to the antiproton. The product antineutron is recognizable by its annihilation properties. Namely, an antineutron on annihilation forms an annihilation star extremely similar to an antiproton star. Instead of reproducing their experimental arrangement, I will show (Fig. 1) a graphical picture of these phenomena as observed in a bubble chamber through the joint efforts of Wilson Powell and his group and my own group (*6*).

Similarly, the antilambda was found by Baldo-Ceolin and Prowse (*7*) in photographic emulsions exposed to a pion beam and was confirmed in the hydrogen bubble chamber. Also the antisigma-zero has been recently seen in a hydrogen bubble chamber by the Alvarez group in Berkeley (*8*).

It is also possible to predict with certainty that some of the nucleonic properties of the antinucleons—specifically the spin, I-spin, third component of the I-spin, and parity—are those shown in Table 1.

But in addition to these interesting questions of systematics of particles, which can be summarized by the diagram shown in Fig. 2, there are problems for which we know much less what to expect because they involve more than general symmetry properties. They require a fairly detailed knowledge of interactions and subnuclear structure, which at present we do not have. Indeed these are the most interesting and challenging problems.

For instance, we know that a nucleon and an antinucleon may annihilate each other, but what are the products of the annihilation? What is their energy? What are the collision cross sections? It is in this direction that we are working now, and here we must be guided mainly by experiment, at least for the time being, and also be prepared for surprises.

Fig. 1. An antiproton enters a propane bubble chamber, and at the point marked with the arrow undergoes charge exchange. The antineutron originates the annihilation star (directly below). Density of propane, 0.42 gm/cm³. Real distance between charge exchange and origin of star, 9.5 cm. T_p at change exchange, ~ 50 Mev. [From Agnew *et al.* (*6*)]

Collision Cross Sections

The first surprise came immediately after the discovery of the antiproton, when we found that this particle has an unusually large collision cross section. This fact has now been studied intensively for some time. The simplest situation occurs in the case of proton-antiproton collisions. There, in addition to the charge-exchange process mentioned above, there are two other possibilities, elastic scattering and annihilation, at least until we reach energies such that inelastic processes (pion production) also become possible. Thus we have three cross sections: for scattering, for annihilation, and for charge exchange. All three have been measured for a wide energy interval, and the results are shown in Fig. 3.

The magnitude of these cross sections is striking when we compare them with those obtained in proton-proton collisions. A tentative theory of this phenomenon has been put forward by Chew (*9*) and his associates and also by Koba and Takeda in Japan (*10*).

The model is based on the Yukawa theory of nuclear interactions in such a way as to stress the analogy between the nucleon-nucleon and the nucleon-antinucleon system. For the nucleon-nucleon system a model consisting of a hard repulsive core of radius about one-third the Compton wavelength of the

pion ($0.45 \cdot 10^{-13}$ cm) surrounded by a pion cloud has been reasonably successful in explaining the experimental results of the scattering and polarization experiments. The pion cloud, which is involved in the interactions at moderate distance, can be treated from first principles of pion theory. The hard repulsive core, on the other hand, is unaccounted for from a pion theoretical point of view and must be introduced *ad hoc* as a phenomenological hypothesis, although the existence of heavier mesons such as the K-mesons may have something to do with it. For a nucleon-antinucleon system the pion cloud of the antinucleon is substituted by its charge conjugate according to the expectations of meson theory, and the medium-range interactions are treated on the basis of this theory. The overlap of the cores, however, is now supposed to bring annihilation instead of strong repulsion. On the basis of this model it has been possible to account for most of the observations made thus far—which, however, do not extend to energies above 1 Bev, where some critical tests of the theory will become possible.

In addition to the total cross sections for scattering, annihilation, and charge exchange mentioned above, the angular distribution on scattering has been measured. Here a large diffraction peak in the forward direction has been found. It is directly related to the annihilation.

The extension of the cross-section studies to complex nuclei has been started. The deuteron was first investigated in the hope of finding information on the neutron-antiproton interaction. Here the data are still very rough, mainly because the subtraction techniques which we were forced to use introduce considerable error. The qualitative feature seems to be that there is not much difference between proton-antiproton and neutron-antiproton collisions.

For heavier nuclei the data from the nucleon-antinucleon collision have been fed into an optical-model treatment, and the results agree with the experimental data as far as they are available. This gives a consistent picture connecting the more complicated case to the simpler one.

There are, however, still some crucial tests to be performed on the p-$\bar{p}$ case in order to validate the Chew model. At high energy, say 2 Bev, the annihilation cross section should be essentially the cross section of the core, and hence considerably smaller than the one observed at lower energy: 10^{-26} cm^2 would

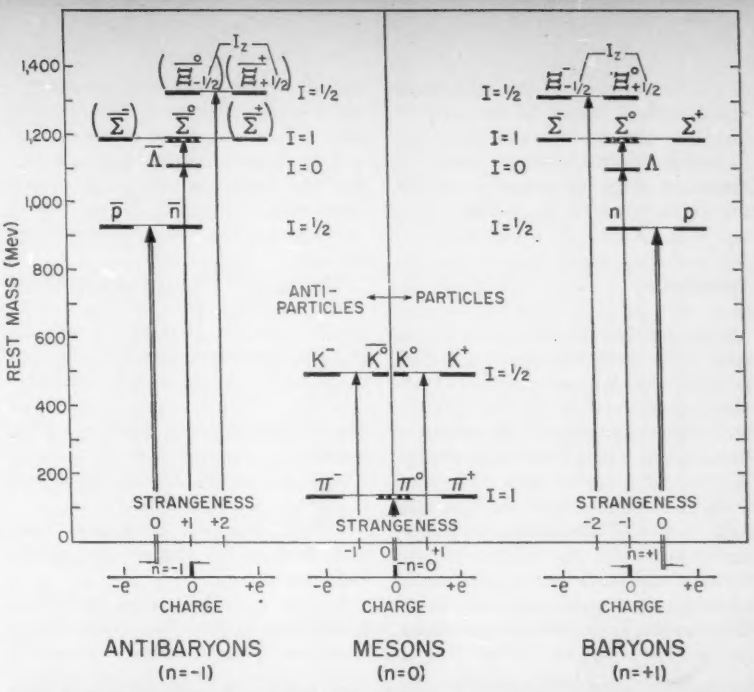

Fig. 2. A diagram showing all strongly interacting particles as known or predicted today. The particles still unobserved are in parentheses. The weakly interacting particles not reported in this diagram are the $\mu^{\pm}$ meson, the electron and positron, the neutrino and antineutrino, and the light quanta. [From Gell-Mann and Rosenfeld, *Ann. Rev. Nuclear Sci.* **7**, 407 (1957)]

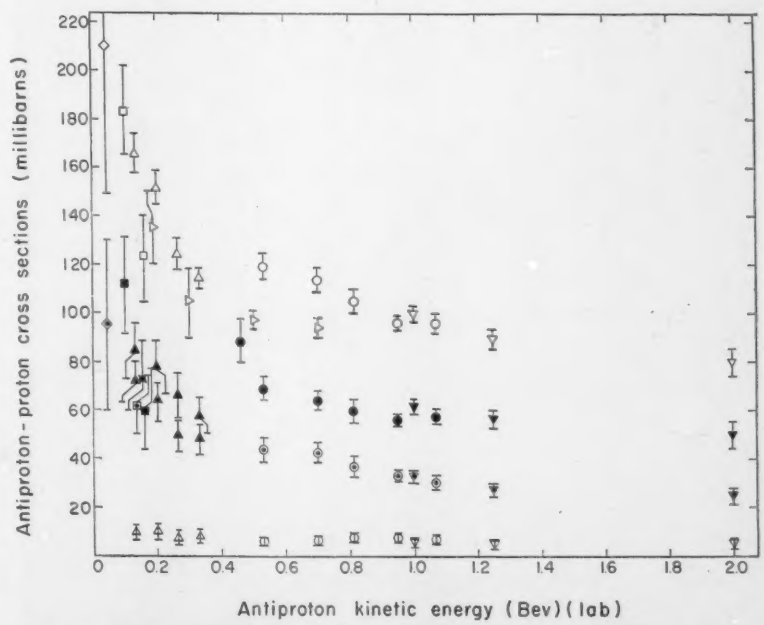

Fig. 3. All $\bar{p}$-p cross sections published up to November 1959. The open symbols are total cross sections; solid symbols are inelastic cross sections (which are due to annihilation only for $T_b < 290$ Mev); open symbols encircling a dot are elastic cross sections; open symbols crossed by a vertical line at the bottom of the figure are charge-exchange cross sections. [The various symbols are referenced as follows: (□) Agnew, Elioff, Fowler, Gilly, Lander, Oswald, Powell, Segrè, Steiner, White, Wiegand, Ypsilantis, *Bull. Am. Phys. Soc.* **4**, 357 (1959); (▽) Armenteros, Coombes, Cork, Lambertson, Wenzel, *ibid.* **4**, 356 (1959); (○) Chamberlain, Keller, Mermod, Segrè, Steiner, Ypsilantis, *Phys. Rev.* **108**, 1553 (1957); (△) Coombes, Cork, Galbraith, Lambertson, Wenzel, *Phys. Rev.* **112**, 1303 (1958); (⊙) Elioff, Agnew, Chamberlain, Steiner, Wiegand, Ypsilantis, *Phys. Rev. Letters* **3**, 285 (1959); (▷) Cork, Lambertson, Piccioni, Wenzel, *Phys. Rev.* **107**, 248 (1957); (◇) Horwitz, Miller, Murray, Tripp, *ibid.* **115**, 472 (1959); (☆) Emulsion results of many authors compiled and averaged by Baroni *et al.*, *Nuovo cimento* **12**, 564 (1959).]

be a generous guess. If this expectation is not fulfilled it will be necessary to look for some other model. I will not go further into the numerous problems connected with cross-section studies and will turn now to the annihilation.

Annihilation

The annihilation process itself has been fairly well investigated experimentally, but the theoretical situation leaves much to be desired. Initially the effort was mainly directed toward establishing the fact that the energy released was $2mc^2$, thus furnishing a final proof of the annihilation. In the early investigations with photographic emulsions carried out in my group (especially by Gerson Goldhaber) and by a group in Rome led by Amaldi, we soon found stars showing a visible energy larger than mc^2 (m is the mass of the proton, c the velocity of light), giving conclusive evidence of the annihilation in pairs of proton and antiproton (*11*).

The observations on annihilation have been performed with many techniques. Initially, immediately after the identification of the antiproton, these particles were stopped in a block of heavy glass, and the showers due to the gamma rays resulting from the decay of neutral pions were observed by Moyer and his co-workers (*12*). This method was not, however, very quantitative.

Photographic emulsions were also exposed to antiprotons at the earliest possible moment. Here we see only the charged annihilation products, although much detailed information is obtainable (see Fig. 4). The great observational effort needed here was shared in a large cooperative experiment in which many laboratories in the United States and in Europe participated (*13*).

Bubble chambers have also been used, both of the propane and of the hydrogen type.

By now we know a good deal about annihilation. It gives rise prevalently to pi-mesons. These, in a time of the order of 10^{-8} second, decay into mu-mesons and neutrinos. The mu-mesons, in a time of the order of microseconds, decay into electrons or positrons and neutrinos, and the electrons and positrons finally recombine to give gamma rays. In a few microseconds the total rest mass of the nucleon-antinucleon pair degrades to particles with rest mass zero, traveling away from the spot of the annihilation with the velocity of light.

Direct annihilation into photons may occur, but this is expected to be rare and thus far has never been observed with certainty.

The reason for this difference between the behavior of electron-positron and nucleon-antinucleon pairs is, of course, that the latter can annihilate not only through the electromagnetic interaction that gives rise to light quanta but also through the specific nuclear interaction whose quanta are the pions. This last interaction is much stronger than the electromagnetic one, and when both are simultaneously present, the effects of the specific nuclear interaction overwhelm those of the electromagnetic interaction, which is the only one available to the electron-positron pair.

The most significant result of the annihilation studies is that the annihila-

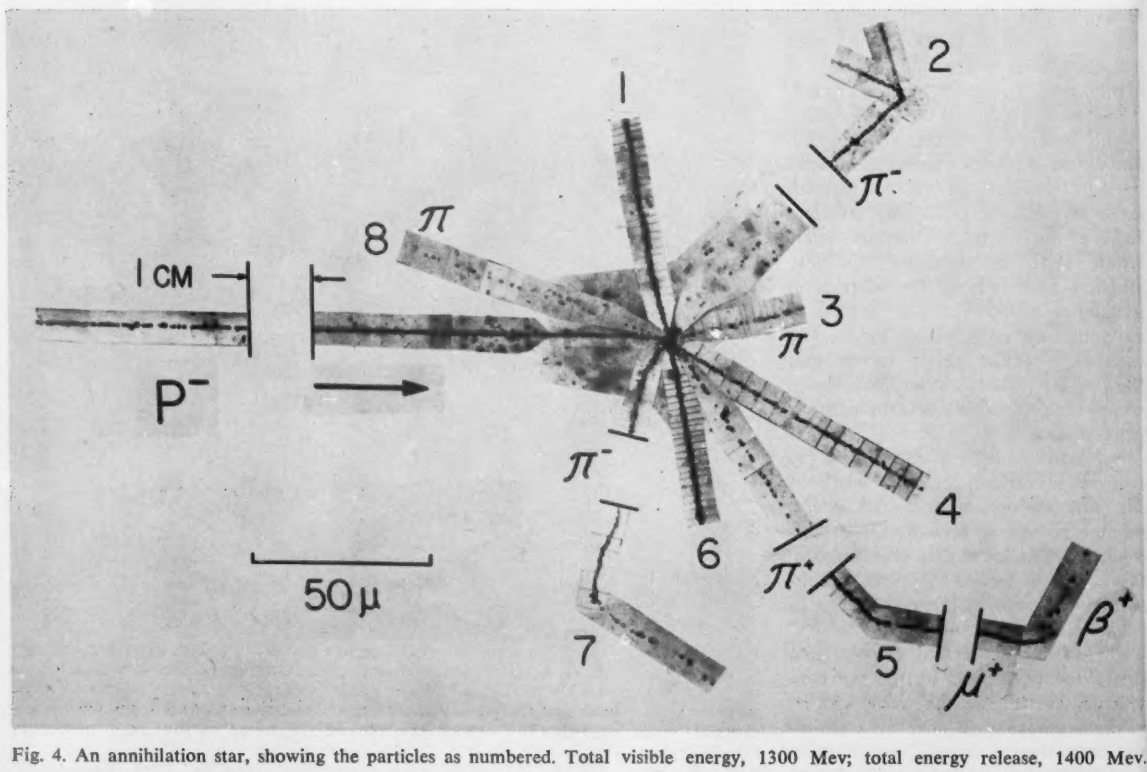

Fig. 4. An annihilation star, showing the particles as numbered. Total visible energy, 1300 Mev; total energy release, 1400 Mev.

No.	1	2	3	4	5	6	7	8
Identity	p(?)	π^-	π(?)	p	π^+	H^3(?)	π^-	π(?)
T (Mev)	10	43	175	70	30	82	34	125

tion process gives rise to an average of 4.8 pions per annihilation, about equally divided among positive, negative, and neutral pions. These pions escape with a continuous energy distribution, the average kinetic energy being about 200 Mev. In about 4 percent of the cases of annihilation at rest, strange particles, K-mesons, are emitted (see Fig. 5).

The escaping pions give rise in complex nuclei to secondary processes, and thus a number of nucleons or light nuclei are also found among the particles emitted on annihilation. Sometimes the relatively rare K-mesons interact, producing a Λ-hyperon, and even more complicated hyperfragments have been observed (Ekspong).

In hydrogen, the multiplicity of the prongs (I am referring of course only to charged particles) for annihilations at rest is given in the following little table:

Charge multiplicity	0	2	4	6	8
Number of stars (Total, 222)	10	89	109	14	0

Naturally, only even numbers of charged prongs may appear because the total charge of the proton-antiproton system is zero.

From the theoretical point of view, we don't yet have an entirely satisfactory picture of the annihilation process. It has been mostly analyzed on the basis of a statistical theory put forward many years ago by Fermi, which does not take into account any detailed mechanism, but only the obvious and necessary features determined by phase space. This theory contains only one free parameter—namely, the volume into which the energy released on annihilation is concentrated at the beginning of the phenomenon. Naturally, this volume is supposed to be the one corresponding to a sphere of radius equal to the radius of action of nuclear forces. If one calculates what is to be expected on this basis one finds a result which is in rather poor agreement with experiment—namely, the multiplicity of pions produced is larger than that predicted by the model. Clearly the average energy and the multiplicity are connected, and hence the average energy also disagrees with the naive statistical prediction. The model can be made to yield correct results by increasing beyond what seems plausible the volume in which the energy comes to equilibrium. Many attempts have been made to refine Fermi's theory and to bring it into agreement with facts. Some of these attempts are very ingenious, and one would wish that there were more success than there is. The ratio between K-mesons and pions is another element of the puzzle that has to be taken into account and seems rather intractable for the time being.

It is, however, hardly to be expected that a purely statistical theory should explain quantitatively the annihilation process, inasmuch as selection rules, strong interactions of the escaping particles, and other important factors completely omitted in the theoretical picture are at work. I think that the future study of the annihilation process, with its bearing on the core of the nucleon—a region of which we know so little—will give some important results. Antinucleons are especially suited for this study because they will exhibit more clearly than other particles the effects of the core.

Antimatter

And now let me say some words on the popular subject of the "antiworld." As early as 1933 Dirac, in his Nobel lecture, said:

"If we accept the view of complete symmetry between positive and negative electric charge so far as concerns the fundamental laws of nature, we must regard it rather as an accident that the earth (and presumably the whole solar system) contains a preponderance of negative electrons and positive protons. It is quite possible that for some of the stars it is the other way about, these stars being built up mainly of positrons and negative protons. In fact, there may be half the stars of each kind. The two kinds of stars would both show exactly the same spectra, and there would be no way of distinguishing them by present astronomical methods."

Fig. 5. Annihilation of an antiproton in carbon, giving rise to a K^0 meson and a Λ^0 hyperon.

We can now add that the proved existence of the antinucleons has very strongly corroborated this possibility, although we also know that the symmetry between electric charges breaks down for weak interactions. As far as astronomical means are concerned, a verification seems impossible in principle, because they depend on electromagnetic phenomena, which are invariant under charge conjugation. It is, however, interesting that the recent important discoveries about beta decay and the neutrino now give a method for looking for antimatter which, while still impossible in practice, is sound in principle, being based on weak interactions which are *not* invariant under charge conjugation. This method, if it could be executed, would solve unambiguously the question of the existence of antiworlds. If we observe a star and from its astronomical characteristics can decide that most of its energy comes from a known cycle, as for example the carbon cycle, which is dominated by beta decays, we can see whether the antineutrinos coming from it are or are not of the same kind as the antineutrinos coming from a pile or from our sun by performing an inverse beta-decay experiment. If it should turn out that they are neutrinos —different from those coming from the sun—then the star is of antimatter (*14, 15*).

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14. As in many investigations in high-energy physics in recent times, this experiment is the result of a large cooperative effort. The credit for the success is shared by many individuals and even by a machine, which was obviously necessary to produce particles above the threshold for nucleon pair production. Since it is impossible to mention all the numerous contributors, I shall limit myself to a few. Oreste Piccioni helped materially in the early planning of the experiment, especially by suggesting the use of magnetic quadrupole lenses. Edward J. Lofgren most ably directed the operation of the Bevatron. Herbert M. Steiner supplied invaluable help during the whole experiment. Tom J. Ypsilantis, our colleague and coauthor, also worked with us all the time. Above all, however, our coauthor and comrade of 20 years of work, Clyde Wiegand, was indispensable and deserves a major part of the credit for the success of our investigation.
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General Theory of Mortality and Aging

A stochastic model relates observations on aging, physiologic decline, mortality, and radiation.

Bernard L. Strehler and Albert S. Mildvan

Although statistics on human mortality furnish one of the most extensive and reliable collections of biological data, general theories to account for the quantitative relationships between age and death rate have not been completely satisfactory. The essential observations which must be taken into account in any general theory of mortality are as follows:

1) The death rate at any age (of most adult human populations and of many populations of animals) may be expressed as the sum of an age-dependent term (Gompertz, *1*) and an age-independent term (Makeham, *2*)—that is, $R = A + R_0 e^{\alpha t}$).

2) In certain environments the Makeham term (A) predominates (for example, wild birds, *3*), while in most human populations (between ages 35 to 85) and in certain animal populations, the Gompertz term predominates (see Fig. 1). The Gompertzian period is followed by a gradual reduction in the rate of increase of the mortality rate (*4*).

3) The rate of decrease of most physiological functions of human beings is between 0.5 and 1.3 percent per year after age 30, and is fit as well by a straight line as by any other simple mathematical function (*5, 6*) (see Fig. 2).

4) Death rates due to certain important specific causes (for example, cancer, tuberculosis, and heart disease) increase exponentially with age similarly to the total death rate (*7, 8*).

5) Continuous exposure of experimental animals to high-energy radiation tends to increase the Gompertz slope (α) by an amount proportional to dose rate, whereas exposure to a single dose of ionizing radiation does not appreciably affect α, but does increase in R_0 proportionally to dose (*9–11*).

Among the recent attempts to postulate mechanisms underlying the Gompertz function or the other generalities given above, or both, are the theories of Jones (*8*), Failla (*12*), Sacher (*13*), Yockey (*14*), and others (*15, 16*). Each theory has certain attractive features but either fails to account for all of the above observations, possesses internal inconsistencies, or makes incorrect predictions. A detailed critique of these and other theories is in press (*17*).

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Statement of Theory

The present theory (*18*) can be summarized as follows:

Postulate 1. An organism consists of a number of subsystems, each of which has a certain maximum ability to restore initial conditions after a challenge (that is, a change in condition due to internal or external energy fluctuations). Death occurs when the rate at which an organism does work to restore the original state is less than that demanded to overcome the effects of a given challenge.

Postulate 2. The magnitudes of challenges (or, more appropriately, the responses required to overcome these challenges) are distributed energetically like a Maxwell-Boltzmann distribution of energies among molecules (*19*). When these postulates are combined with the Gompertz equation, a number of consequences follow (*20*).

In this article, these predictions and consequences of the theory are enumerated and tested against observation wherever possible. The theory appears to relate simply and satisfactorily the Gompertz function and physiological capacity measurements versus age, as well as a number of other demographic observations. It makes several predictions which subsequent analyses of mortality and physiological data have substantiated. We have been unsuccessful in showing any major inconsistencies between this theory and observation.

The following definitions are used:

1) *Aging.* The inherent process(es) whereby organisms exhibit a gradual change in their physical, chemical, or physiological properties after reproductive maturity. These changes result in a gradual increase in the probability of death in the organisms' normal environment.

2) *Gompertz equation.*

$$R = R_0 e^{\alpha t} \qquad (1)$$

This is a kinetic equation describing the probability of death of many populations as a function of age. R is the rate at any time t, R_0 is the hypothetical rate at zero time, and α is the Gompertz coefficient, which describes the rate of increase of the exponential term.

Makeham's modification of Gompertz' equation,

$$R = A + R_0 e^{\alpha t}, \qquad (2)$$

applies to aging populations which are also characterized by high age-independent death rates. In Eq. (2), A is a measure of the age-independent death rate.

3) *Vitality.* Vitality is defined as the capacity of an individual organism to stay alive, as measured by an appropriately weighted average of the maximum rate of work output (power output) less the basal power output of all of the functional modalities contributing to survival in the normal environment. This weighted mean of an individual's vitality at a given age is designated as V^t. The hypothetical value of V^t at $t = 0$ is V_0^t. Similarly, V and V_0 represent the averages of the above quantities for a population of uniform age. This concept of vitality is similar to one set forth by Medawar (*21*).

4) *Maxwell-Boltzmann distribution* (*19*).

$$n/n_t = k(E/RT)^{\frac{1}{2}} e^{-E/RT} \qquad (3)$$

when $E \geq RT$.

For $E >> RT$, the equation can be simplified to

$$n/n_t \doteq Ke^{-E/RT}, \qquad (4)$$

Where E is the energy of a state of a molecular system, T is the absolute temperature, R is the gas constant, n is the number of molecules with energy equal to or greater than E, and n_t is the total number of molecules.

Equations 3 and 4 describe the distribution of challenges to molecular bonds. Analogous equations derived from the postulates of the theory described in this article appear below (Eqs. 7 and 8), in which the terms E and RT are supplanted by the analogous expressions V and ϵD, respectively. D is a measure of the relative deleteriousness of an environment, whereas ϵ is a constant chosen so that the quotient $V/\epsilon D$ is unitless. In addition, ϵD is a measure of the average demand for energy expenditure over and above the basal level.

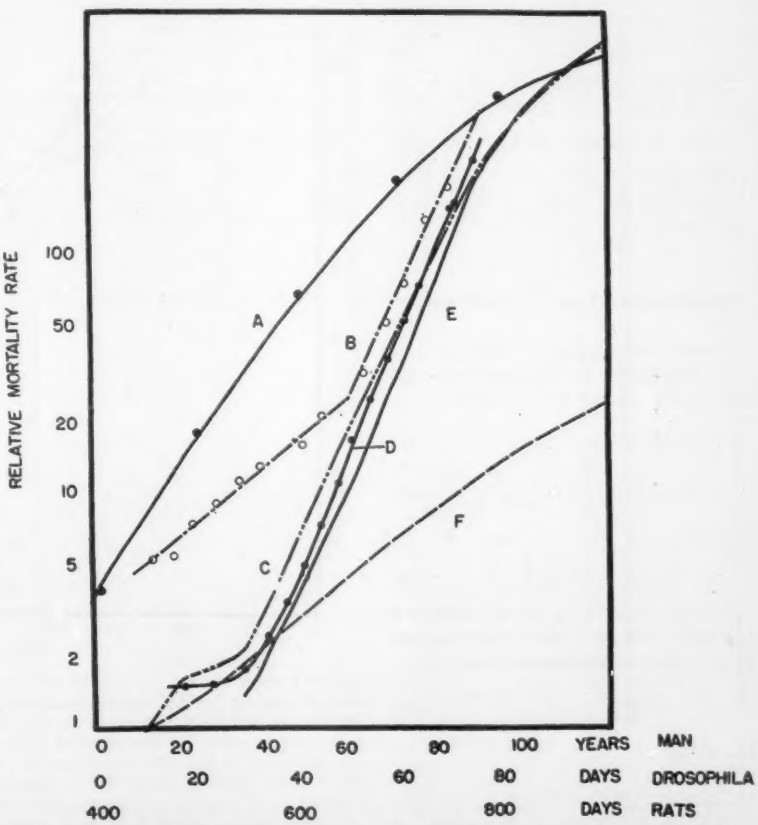

Fig. 1. Gompertz plots of mortality rates for various species. *A*, male rats (*26*); *B*, human male (Egypt 1947) (*22*); *C*, human male (U.S., white—North Central Division, 1949–51) (*4*); *D*, human male (Sweden, 1953) (*22*); *E*, human female (U.S., white—North Central Division, 1949–51 (*4*); *F*, *Drosophila melanogaster* (*29*).

5) *Attrition coefficient.* The attrition coefficient B is the fractional loss each year of original vitality V_0.

$$B = b + f(D), \tag{5}$$

where b is the attrition coefficient due to normal aging and $f(D)$ is the attrition coefficient due to environmental factors (see also Medawar, *21*).

Our theory may be stated mathematically as follows:

Postulate 1.

$$R = CX, \tag{6}$$

where R is the mortality rate, C is the total number of challenges per unit time, and X is the fraction of challenges equal to or greater than V.

Postulate 2.

$$X = k'(V/\epsilon D)^{\frac{1}{2}} e^{-V/\epsilon D} \tag{7}$$

or

$$X \doteq K' e^{-V/\epsilon D} \tag{8}$$

Substituting Eq. 7 in Eq. 6 and using Eq. 1 (that is, a Gompertz function), we obtain

$$\begin{aligned} R = R_0 e^{\alpha t} &= Ck'(V/\epsilon D)^{\frac{1}{2}} e^{-V/\epsilon D} \\ &= k\,(V/\epsilon D)^{\frac{1}{2}} e^{-V/\epsilon D}. \end{aligned} \tag{9}$$

Similarly, substituting Eq. 8 in Eq. 6 and using Eq. 1, we obtain

$$R = R_0 e^{\alpha t} \doteq K e^{-V/\epsilon D}. \tag{10}$$

Equations 9 and 10 are the basic equations of our theory. All subsequent relationships are derived from them.

Predictions and Tests of the Theory

Linear loss of vitality with age.

1) Derivation: Taking logarithms and solving Eq. 10 for V, we obtain

$$V = \epsilon D \ln (K/R_0) - \epsilon D \alpha t \tag{11}$$

$$= \epsilon D \ln (K/R_0) \left[1 - \frac{\alpha t}{\ln (K/R_0)}\right]. \tag{12}$$

When $t = 0$,

$$V = V_0 = \epsilon D \ln (K/R_0). \tag{13}$$

Since α, K, and R_0 are constant, we can define the term containing them as B (the attrition coefficient); that is,

$$B \equiv \frac{\alpha}{\ln (K/R_0)}, \tag{14}$$

and thus

$$V = V_0(1 - Bt). \tag{15}$$

2) Prediction: From Eq. 15 it follows that vitality should be lost at a constant rate.

3) Test: This prediction is in agreement with observation 3 in the introduction (see Fig. 2 also).

Inverse relationship between R_0 and α.

1) Derivation: Rearranging Eq. 14, we obtain

$$\ln R_0 - \ln K = -(\alpha/B) \tag{16}$$

2) Prediction: Contrary to the intuitive notions relating the values of R_0 and α, the theory predicts that a high initial mortality rate (presumably characteristic of an inhospitable environment) should be associated with a low rate of increase (α) of mortality rate. In its most simple form the theory predicts that a straight line will be obtained if $\ln R_0$ is plotted against α, whose slope is $-1/B$ and whose intercept is $\ln K$ (if B is a constant).

3) Test: The extent to which natural human populations approximate this relationship is illustrated in Figs. 3 and 4, in which the individual points represent the values of R_0 and α derived for various countries from the United Nations *Demographic Yearbook* for 1955 (*22*, *23*).

Figures 3 and 4 clearly indicate that both B (which measures the fractional loss of vitality per year) and K (which, according to this formulation, measures the total number of challenges per unit time regardless of their magnitudes) are essentially constant. The constancy of K is a necessary consequence of postulate 1 (and Eqs. 6 to 10). The present observation is thus consistent with this assumption. More surprising is the fact that B appears to be nearly constant

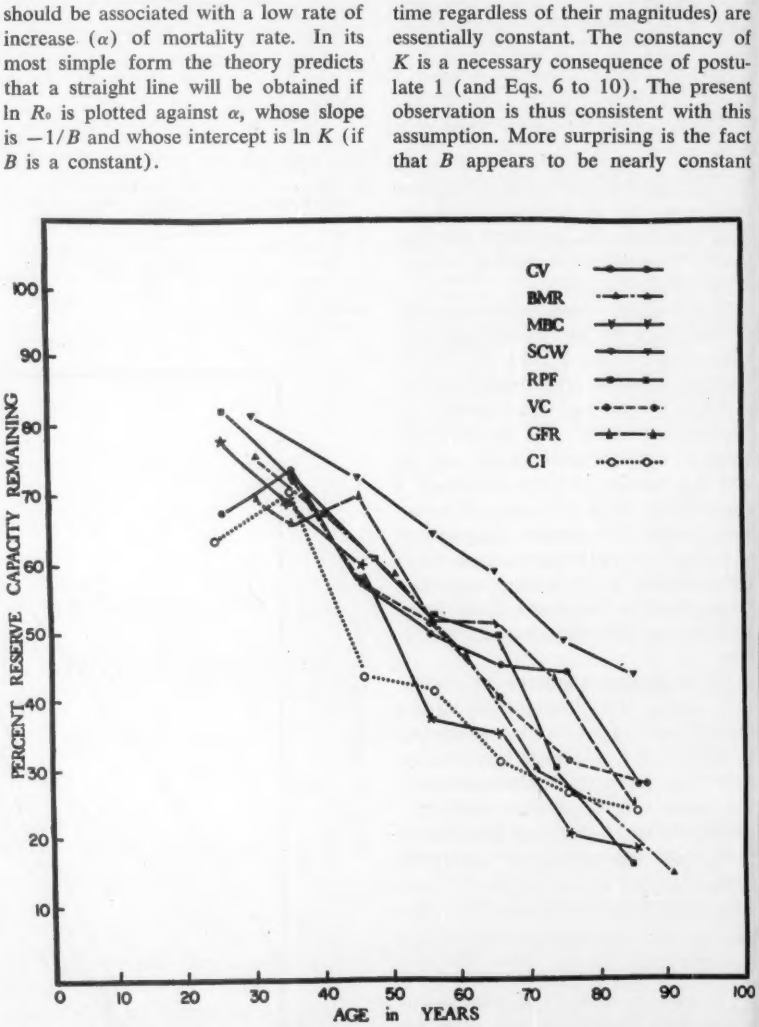

Fig. 2. Percent of reserve capacity of a number of physiologic functions remaining at various ages in human males (calculated from the data of Shock *et al.*, *5*, *6*, *30*). The decade average values for each physiologic function were divided by the calculated initial reserve value for that function. This initial reserve for each physiologic function was measured by the difference between the value of the function extrapolated to age 0 and a lower limiting value of the function. This lower limiting value, in turn, was assumed to be equal to the lowest measured individual value. Independent techniques of estimating this lower limit gave comparable results. (See *17* for a more detailed treatment.) *CV*, nerve conduction velocity; *BMR*, basal metabolic rate; *MBC*, maximal breathing capacity; *SCW*, standard cell water; *RPF*, standard renal plasma flow (Diodrast); *VC*, vital capacity; *GFR*, standard glomerular filtration rate (inulin); *CI*, cardiac index.

regardless of the environment. B was defined in Eq. 5 as being made up of a constant term, b, and an environmental term, $f(D)$. It thus appears that B is dominated by b, or in other words that the rate of loss of vitality during the aging process is largely independent of the environment.

Prediction of the value of the attrition coefficient (B).

Method 1. From the relation of R_0 and α for various countries. If one uses the data of Fig. 3 and Eq. 16, the value of B derived from the over-all slope is about 1.46 percent of V_0 per year. The value of B for the eight highest α countries is about 1.27 percent of V_0 per year, which suggests the possibility of slight curvature. The corresponding range of K values is 0.05 to 0.15 per year. Since real mortality rates approach 0.6 per year in extreme old age, it is clear that the former value is too low and that the real value of K is greater than 0.6.

Method 2. From the relation of R_0 and α, assigning a value of K ($K = 1$) and using Eq. 18. Values for B averaging 0.0097 and ranging from 0.0086 to 0.0111 were obtained. The values for individual countries are listed in Table 1.

Method 3. From the terminal curvature of mortality rate curves. From Eq. 9, which more closely gives the rate to be expected when the reserve capacity (V) approaches the average demand (ϵD), and using Eq. 15, we obtain

$$R = k\ [(V_0/\epsilon D)\ (1 - Bt)^{\frac{1}{B}} e^{-V_0/\epsilon D} \times e^{(V_0/\epsilon D)Bt}, \quad (17)$$

and when $t = 0$

$$R_0 = k\ (V_0/\epsilon D)^{\frac{1}{B}} e^{-V_0/\epsilon D}, \quad (18)$$

Substituting Eq. 15 in Eq. 10, we obtain

$$R = Ke^{-V_0/\epsilon D}\ e^{(V_0/\epsilon D)Bt} = R_0 e^{\alpha t}. \quad (19)$$

Thus

$$\alpha = (V_0/\epsilon D)B$$

and

$$\alpha/B = V_0/\epsilon D. \quad (20)$$

Substituting α/B for $V_0/\epsilon D$ in Eq. 18, we obtain

$$R/R_0 = (1 - Bt)^{\frac{1}{B}}\ e^{\alpha t}. \quad (21)$$

Squaring and solving for B, we obtain

$$B = \frac{1 - (R/R_0 e^{\alpha t})^B}{t}. \quad (22)$$

Test: The values of B calculated from methods 1 and 2 range from 0.92 to 1.37 percent per year, which is in reasonable agreement with the observed rate of loss of human physiologic functional capacities (0.5 to 1.3 percent per year). (See observation 3.) (For a detailed analysis, see Mildvan and Strehler, *17*.)

We have also calculated B values using Eq. 22 and ages over 100 years (*4*). Although the values calculated range from 0.003 to 0.009 per year,

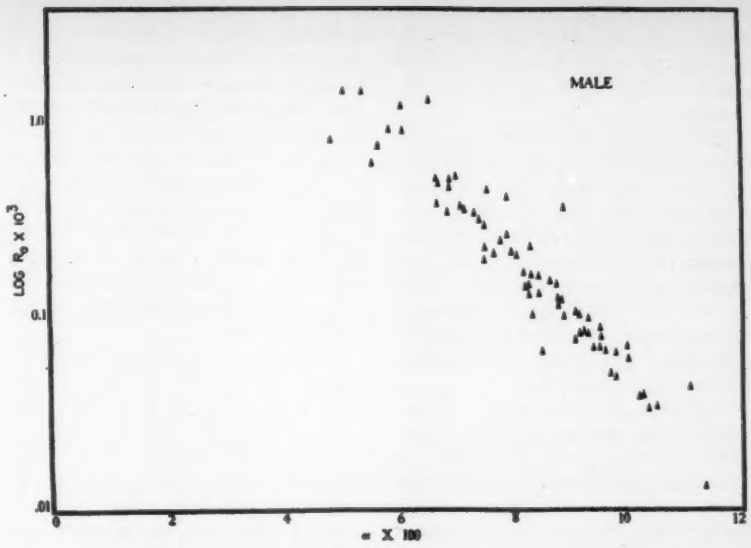

Fig. 3. Gompertz slope (α) versus logarithm of extrapolated hypothetical mortality rate at age 0 ($\ln R_0$) for males of all countries for which adequate data are available (*22*). Each country's mortality rate was plotted individually on semi-log paper. A straight line was drawn between points from 35 to 85 (or 50 to 70, if large departures from linearity occurred), and the slopes and intercepts were measured or calculated. Only a few countries, whose Gompertz plots exhibited great irregularities, were excluded.

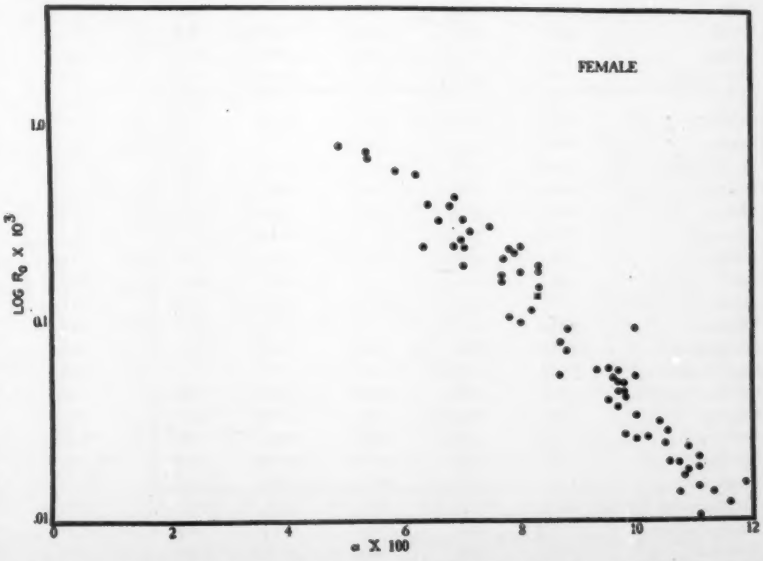

Fig. 4. Gompertz slope (α) versus logarithm of extrapolated hypothetical mortality rate at age 0 ($\ln R_0$) for females of all countries for which adequate data are available (*22*). See legend to Fig. 3 for technique of calculation.

these results are, in all likelihood, fortuitous, since the shape and magnitude of curvature depart from the predicted shape and magnitude by considerable amounts.

Therefore, Eq. 22 cannot be applied to human populations because variability among individuals and subpopulations, combined with inherent inaccuracies in the calculated and reported age specific rates, at advanced ages, will produce unpredictable departures from the ideal. It is hoped that an independent test can be developed on some experimental and more homogeneous population of organisms.

Prediction of quantitative relationship between average demand and reserve vitality of young individuals. In an "ideal" environment an organism would need to expend energy at only the basal rate required for maintenance in the absence of challenges. In real environments, there are demands over and above this basal level which we here define as ϵD, in which D measures the relative deleteriousness of the milieu. In the present analogy V_0 represents the amount by which the maximum rate of energy expenditure of a young person exceeds the basal rate of energy dissipation (appropriately weighted). Particularly, it should be kept in mind that V and V_0 (vitality) include only the reserve capacity of an organism to do work in overcoming challenges to its existence. It does not include the work it must do to maintain itself in the absence of challenge. The latter, appropriately weighted, is the basal work rate.

$V_0/\epsilon D$ is then the ratio of maximum vitality (V_0) to the average demand (ϵD) in excess of the basal work rate, a ratio which the theory has enabled us to estimate, even though we cannot make estimates of the value of V_0 and ϵD independently. Using Eq. 20, $\alpha/B = V_0/\epsilon D$, we can calculate that this ratio for various national environments ranges from about 7.0 to about 11.0 in the worst and best environments, respectively (see Table 1).

Test: For the three functions which we were able to test by analysis of values in the published literature, the ratio $V_0/\epsilon D$ ranges from 7.9 to 32 (see Table 2). The cardiac index and heat output calculations are in good agreement with the values of $V_0/\epsilon D$ calculated from mortality figures; the ventilation rate figures are not.

Relative deleteriousness (D values) of various national environments. From Eq. 20 one can also estimate the relative D values of various countries. The values thus obtained are illustrated in Table 1, in which the most favorable present national environment is assigned a value of 100. Note that the countries with the most extreme values differ from each other only by about a factor of 1.5 in the average stress magnitudes (ϵD). Unfortunately we have not been able to estimate relative deleteriousness by independent objective means.

Prediction of relationship between Gompertz slope, R_0, and radiation exposure. We make the assumption that the total irreparable damage (d) done by radiation exposure is proportional to the total radiation dose (r_t). That is,

$$d = \beta r_t = \beta\rho t, \tag{23}$$

where ρ is the dose rate. The corollary assumption is made that this damage is reflected in a corresponding loss of vitality. Thus,

$$V = V_0(1 - Bt - \beta r_t) \tag{24}$$

or

$$V = V_0[1 - (B + \beta\rho)t] \tag{25}$$

for continuous exposure from $t = 0$. Substituting Eq. 24 and Eq. 25 into Eq. 10 of our theory, we obtain

$$R = Ke^{-(V_0/\epsilon D)(1 - Bt - \beta r_t)} \tag{26}$$

and

$$R = Ke^{-(V_0/\epsilon D)(1 - Br_t)}e^{(V_0/\epsilon D)Bt} \tag{27}$$

for a single dose and

$$R = Ke^{-V_0/\epsilon D}e^{(V_0/\epsilon D)(B + \beta\rho)t} \tag{28}$$

for continuous exposure from $t = 0$.

From these relationships (Eqs. 27 and 28),

$$R_0 = Ke^{-(V_0/\epsilon D)(1 - Brt)} \tag{29}$$

for a single dose and

$$\alpha = (V/\epsilon D)(B + \beta\rho)$$

for continuous exposure from $t = 0$.

In other words, the theory (combined with the subsidiary assumptions above) predicts that the extrapolated ln R_0 for a single dose will increase proportion-

Table 1. R_0, α, and derived quantities for human males of various countries.

Country	Year	$R_0 \times 10^3$	α	B^* (K=1)	$\frac{\alpha}{B}$ (K=1)	Relative D value; (D_{Norway}=100)
Alaska	1950	0.247	0.0774	0.00934	8.3	130
Algeria (European)	1948	.335	.0735	.00935	7.9	136
Algeria (Moslem)	1948	.820	.0612	.00864	7.1	152
Argentina	1947	.364	.0724	.00908	8.0	134
Australia	1953	.057	.0948	.00970	9.8	110
Austria	1953	.0450	.1010	.01016	9.9	109
Belgium	1953	.0620	.0943	.00974	9.7	111
Brazil	1950	.640	.0671	.00914	7.4	146
Canada	1953	.0370	.0986	.00970	10.1	106
Costa Rica	1950	.0780	.0935	.00990	9.4	114
Chile	1952	.205	.0818	.00975	8.4	128
Denmark	1954	.0340	.102	.0110	9.2	116
Egypt	1947	.150	.0865	.00977	8.8	122
El Salvador	1950	.172	.0788	.00912	8.6	125
Finland	1953	.133	.0866	.00971	8.9	120
France	1953	.0910	.0917	.00986	9.3	115
Germany	1937	.0900	.0896	.00963	9.3	115
Ireland	1953	.0780	.0953	.0101	9.4	114
Italy	1951	.030	.103	.0093	10.7	100
Japan	1953	.062	.0963	.00994	9.7	111
Mexico	1950	.310	.0731	.00908	8.1	133
Netherlands	1953	.038	.0980	.00965	10.2	106
New Zealand	1954	.0318	.1030	.00997	10.3	104
North Ireland	1954	.0560	.0950	.00971	9.8	110
Norway	1953	.0245	.104	.00970	10.7	100
Portugal	1954	.0540	.100	.0102	9.8	110
Sweden	1953	.037	.0987	.00970	10.2	106
Switzerland	1953	.069	.0929	.0097	9.6	112
Trinidad and Tobago	1953	.022	.119	.0111	10.7	100
United States	1953	.200	.0783	.0092	8.5	126
Venezuela	1950	.376	.0684	.00863	7.9	135
West Germany	1953	.062	.0943	.00973	9.7	111

* Statistical constants related to B when $K = 1$: number, 32; mean, 0.00966; standard deviation, 0.000469; standard deviation/mean, 0.0485; standard error of mean, 0.0000828.

Table 2. Calculation of reserve ratio $V_0 / \epsilon D$ for several physiological functions.

Function	Units	Basal value	Av. value	Maximal value (av.)	Max. basal / av. basal $= \frac{V_0}{\epsilon D}$	References
Cardiac index	lit. /M² min	4.3	4.6–5.0*	9.8	7.9–18	(*31, 32*)
Ventilation rate	lit./min	4.9	9.15†	68.5–140.5	15–32	(*30, 33*)
Heat output	kcal/hr	65	104–146‡	600	6.6–13.7	(*31*)

* Average cardiac index calculated on the basis of 2500 to 3500 kcal per day according to the observed functional relation between cardiac index and heat output (*33*). † Average ventilation rate calculated on the basis of 2900 kcal/day (*33*) and of an assumed respiratory quotient of 0.9 ‡ Average heat output calculated on the basis of 2500 to 3500 kcal/day.

ally to the dose and that the Gompertz slope (α) will be increased proportionally to the dose rate for continuous exposure.

Test: This is in agreement with observation 5 (see Fig. 5).

Discussion

The present theory of mortality is based on the kinetics of death of populations and entails the tacit assumption of approximate equivalence among the various individual members of a population. However, it is clear that considerable variability does exist among individuals with respect to their physiological and physical properties and maximum (*5, 6*) performance capabilities, and we shall therefore here consider the potential effects of individual variability on the mortality behavior of populations in terms of the present theory.

Variability among individuals might be expected to arise from difference either in their genetic constitution or in their environment. Each of these factors contributes to the several terms in the equations.

Thus, V_0' (for an individual) is the resultant of the interaction of genetic make-up and the environmental factors contributing to maturation. Similarly, B, which represents the fractional loss of vitality per year, is made up of an intrinsic (presumably genetically determined) term and an environmental term.

Variability in V_0' and B' within a population is capable of producing curvature in the Gompertz plot. When ϵD is assumed to be a constant, it is clear (from Eq. 20) that subpopulations will have identical α's only when the product $V_0 B$ is a constant. Thus, a distribution of values of either V_0' or B', when $V_0' B'$ is not constant, will result in a curve with a gradually increasing slope (α) (since the sum of several increasing exponentials with different α's always yields a curve whose slope increases with time).

On the other hand, there may be an opposing effect of variability in either B' or V_0' (assuming either that BV_0 is constant or is not constant). This tendency of the average value of α in a heterogeneous population to decrease at great ages is a consequence of the fact that those individuals are most likely to die who have the smallest V at any age. The death of such low-vitality individuals tends to leave the average vitality of the remainder at a higher value than it would have if the weaker individuals had not been removed.

Since the death rate of a total population is determined by the average V, the Gompertz slope will approach the value of the lower mortality rate subpopulation. This may yield a gradual decrease in α. These two opposing effects are illustrated in Fig. 6.

In addition, it can be shown from Eq. 17 that one would expect a gradually decreasing value of α with advancing age.

Since real populations exhibit remarkably linear Gompertz kinetics up to great ages, we must assume either that variability is not great compared to ϵD or that the subpopulations are made up of such mixtures of the above three possibilities that opposing effects tend to cancel each other out. Thus, individual variability is not inconsistent with the present theory. The extent to which individual variability contributes to the shape of mortality curves cannot now be evaluated.

Simms (*16*) and Jones (*8*) have pointed out that a number of individual causes of death also exhibit Gompertz kinetics. Jones has emphasized the approximate similarity of slopes of various cause-specific mortality curves. We have plotted and examined the cause-specific death rates for the United States and have found some, although not general, confirmation of this observation. Of the four major causes of death (heart disease, cerebrovascular accidents, neoplasms, and accidents), the second and third have almost identical slopes and intercepts, whereas the principal cause of death (heart disease) has a lower intercept but greater slope. The sum of these curves is not a good straight line.

Several conclusions can be drawn from the above. First, although the death rates due to specific disease are not constant fractions, throughout life, of the total death rate, their slopes and rates are so adjusted that their sums approximate straight lines. This is apparently true because of the inverse re-

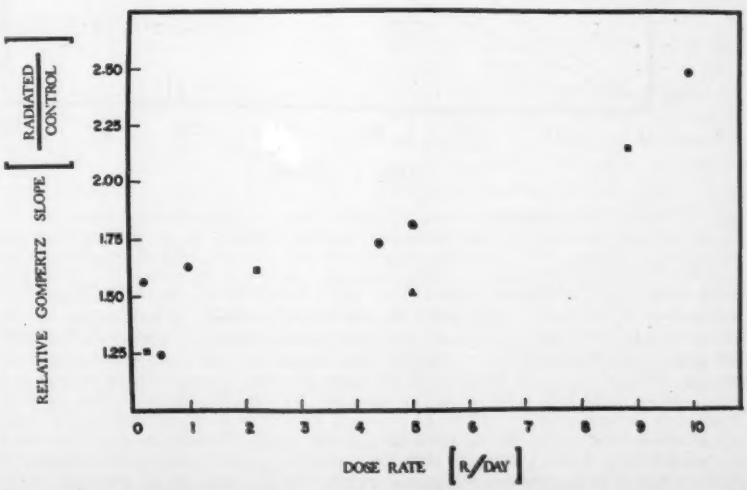

Fig. 5. Effect on Gompertz slope of chronic irradiation exposure of rats, after original data from: (circle) Furth *et al.* (*10*); (square) Dowdy *et al.* (*34*); (triangle) Brues and Sacher (*35*). For a more detailed discussion, see Berlin and DiMaggio (*28*).

lationship between R_0 and α of cause-specific rates; this relationship tends to compensate for the curvature otherwise to be expected.

It appears that the V_0's and B's for important cause-specific death rates may have been subjected to evolutionary selection in such a manner that total deaths due to the three major causes are similar near the close of the normal reproductive lifetime. As Williams (*24*) has pointed out, it is to be expected that there would be a strong selection pressure against any genetically dependent single cause of death.

Finally, it appears possible that at least some of the increased slope of the mortality rate curve in "good" environments is related to the predominant contribution of heart disease in these countries.

The fact that the present theory predicts the observed relationships between dosage, α, and R_0 is a valid test of the theory. It is not, however, a test of the theory that radiation accelerates aging. Radiation, like any other noxious agent that produces permanent damage to organisms or cells, would be expected to contribute to mortality as observed. But radiation damage may affect systems adversely that normally might be relatively unaffected by time, and vice versa (*25–28*). A test of the theory of the equivalence of time and radiation awaits a detailed description of the intimate mechanisms and details of the changes produced by each process.

It is of interest that the theory predicts that the maximum lifetime attainable in a homogeneous population will be approximately $1/B = 103$ years for the calculated $B = 0.0097$. Compare this figure with the maximum ages reliably reported for human beings—110 to 115 years.

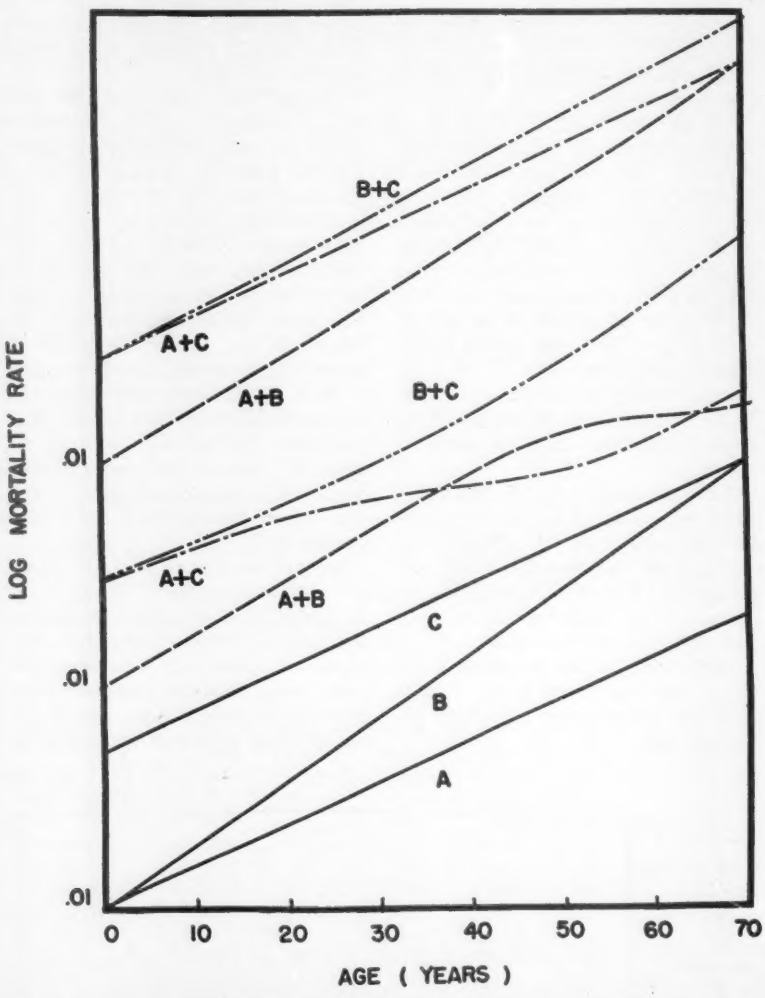

Fig. 6. Summation of mortality rates of various pairs of Gompertzian subpopulations. Three homogenous Gompertzian subpopulations are depicted as A, B, and C at the bottom of the figure. The mortality rates (deaths per year per individuals alive at the beginning of the year) varied as follows between ages 0 and 70: curve A, 0.01 to 0.2; curve B, 0.01 to 1.0; curve C, 0.05 to 1.0. The behavior of the sum of various combinations of A, B, and C, as calculated by two different methods, is also shown: At the top of the figure the average rates are calculated without taking into account the selective loss of individuals from the subpopulation having the higher mortality rate. Note that the slope of $A + C$ is identical to the slope of A or C, whereas the $A + B$ and $B + C$ curves show very slight concavity upwards. By contrast, the more real situation depicted in the curves in the middle of the figure shows the effect of the selective elimination of individuals (by death) from one of the subpopulations on their combined mortality curve. Note particularly that the curve for $A + B$ is highly reminiscent of the behavior of many human and animal populations (particularly at great ages). On the other hand, $B + C$ yields a curve which displays considerable concavity upwards, whereas $A + C$ gives a curve with an inflection part way up and with approximately equal slopes before and after.

Summary and Conclusions

1) A theory of the kinetics of death is presented which is based upon the experimentally determined Gompertz function and the two following postulates: (i) The distribution of stress magnitudes is a Maxwell-Boltzmann distribution; (ii) an organism dies when stress magnitude exceeds the organism's maximum ability to compensate therefor.

2) The theory predicts a zero-order loss of function versus age. This is borne out in human males by independent observation.

3) The theory permits several independent calculations of the value of B, the percent of loss per year of physiologic function. The calculated values range from 0.9 to 1.4 percent per year and agree closely with the observed rates in human males.

4) The theory predicts an inverse linear relationship between Gompertz slope and ln R_0 (intercept), which is closely confirmed by observation.

5) The theory predicts that the mean ratio of maximum reserve capacity to average demand lies between 7 and 11. Independent physiologic measurement data are in reasonable agreement with these values.

6) The theory predicts the observed effects of prolonged or "instantaneous" exposure of experimental animals to ionizing radiation.

7) The relative deleteriousness (D) of various national environments can be calculated. They have been found to differ by approximately 50 percent. We have been unable to make an independent test of these relative values.

8) Despite the fact that it is derived for a homogeneous population the theory is shown to be not inconsistent with individual variability within a population.

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20. The present theory is expressed in terms of energy fluctuations arising internally or externally. This dimensional system is chosen because: (i) death undoubtedly cannot occur without the occurrence of a change in structure or physiological state, which change is the result of energy expenditure; (ii) death will occur if the capacity of the system to restore conditions necessary for life is exceeded; (iii) this restoration of original conditions requires the expenditure of energy of a certain kind at a certain minimum rate. Thus, an organism lives or dies according to whether its maximum power output in the challenged modality is sufficient to overcome the disruptive influence of the challenge. It is also clear that any parameter exhibiting an inverse logarithmic relationship between magnitude and frequency (coupled with a linearly decreasing resistance thereto) would be sufficient to generate the observed mortality behavior. Such a functional relationship would result from the square of any variable that exhibits a Gaussian distribution—for example, molecular velocity. Thus, the agreement between prediction and observation does not "prove" the validity of the energy analogy. However, attempts to postulate and measure underlying variables other than energy or its equivalent have thus far not been successful. For the above reasons we have emphasized the energy analogy as being the most appropriate one at present.
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Roy Chapman Andrews, Explorer

Some sixty years ago Roy Chapman Andrews, the son of a rural druggist in southern Wisconsin, chanced upon *A Handbook of North American Birds* by Frank M. Chapman of the American Museum of Natural History. The book, and the author of that book, were to influence most profoundly the life of Roy Andrews, just as decades later the writings of Andrews were to cast their spell over the lives of many boys entering into manhood. Young Andrews had a burning desire to meet Chapman, and eventually he did meet him, in the summer of 1906.

Andrews had at that time just completed his undergraduate career at Beloit College, and he faced the world with restless energy. To him, a young man fond of the outdoors and a veteran of numerous camping trips in his native state, the American Museum of Natural History and Frank Chapman were the two poles of a magnet that attracted him with irresistable force to New York. It is an oft-repeated tale, how he approached the director of the Museum and asked for employment, how he was told that there were no staff openings available, and how he took a menial job as general attendant and handyman in the department of preparation, where he mixed clay and scrubbed floors.

His enthusiasm for his work and for the museum were immediately apparent, and he very quickly advanced within the institution. In 1908 he went to British Columbia to make field studies of whales, an endeavor that set the pattern of his life for the next 8 years, which he spent in active and diverse field work on Pacific whales. As a result of these studies he published two monographs and several short papers on the Cetacea, and these were in essence the totality of his research publications.

It became apparent to him early in his career that research was not his major interest; rather, he developed an overwhelming desire to carry on field work and exploration. This was in part the result of his youthful camping days

in Wisconsin, in part an expression of his restless spirit and his active body. It was not easy for him to sit still, for he was not the contemplative type but a lover of action and of the out-of-doors. He wanted to go to the far places of the earth, not for the sake of going, but for a definite purpose—to conduct scientific explorations in little-known lands.

So it was that in 1916 he turned his eyes toward Central Asia. He became obsessed with the idea of exploring what was then the mysterious land of Mongolia, to search for scientific treasures that were buried in that almost unknown country. Undoubtedly he was stimulated in this interest by the theories of William Diller Matthew, who regarded Central Asia as the center of origin for most of our mammalian orders, and of professor Henry Fairfield Osborn, who believed this region to be the birthplace of primitive man. Consequently, in the years just before 1920 he went on two very modest reconnoitering expeditions in areas adjacent to Mongolia, to feel his way toward a larger goal.

Then, with the beginning of the third decade of our century he entered into the planning and the arduous work that were to come to fruition in the famous Central Asiatic Expeditions of the American Museum of Natural History. Those were the days before large foundation and government grants, and money had to be raised individually and privately. Andrews had the talent to do this. By the end of 1921 he had amassed a fund sufficient to begin work in Central Asia on the scale that he envisaged.

It was to be no ordinary trip by two or three trained scientists and their retainers. Andrews planned and put into the field a large and elaborate expedition, staffed by outstanding authorities in geology, paleontology, zoology, botany, and anthropology, with technicians and numerous field assistants. He had the very original concept of operating with a contingent of motor cars supported by a large camel caravan. The camels crossed the Gobi on a predetermined course, loaded with containers

Roy Chapman Andrews

full of gasoline and oil. At stated places supply dumps were established, and these were visited by the motor fleet. It was a complex problem in logistics and in what might be called land navigation, and it was nicely solved. With the cars the scientific staff could range far and wide across the Gobi, covering vastly more ground in a season than had ever been covered before by scientific explorers working under similar conditions.

The first expedition, in 1922, was eminently successful and was followed by other expeditions, in 1923, 1925, 1928, and 1930. Andrews and some of his staff spent the intervening winters in Peking. The results of these expeditions have been published in many scientific contributions, and researches are still continuing on the collections that were made during those years. The expeditions did not discover the earliest men (years later such beings, the australopithecines, were to be found in Africa) but they did find many fossil reptiles and mammals, including some spectacular discoveries of dinosaurs, the first association of dinosaurs with their eggs, the first finds of placental mammals in the Cretaceous deposits along with the dinosaurs, various important Cretaceous and Tertiary vertebrate faunas, and archeological sites of importance. In addition, the expeditions made zoological and botanical collections, as well as an important series of topographic and geologic maps of Mongolia.

There were no more trips after the expedition of 1930; unrest in Asia made further work in Mongolia impossible. Thus Andrews, who had been in Asia almost continuously for a decade, returned to New York. The field work in Mongolia had revealed Andrews' ability to supervise large projects and to work with men, so it was no surprise when in 1933 he was made director of the American Museum of Natural History. He served in this capacity until 1941, when he retired.

After his retirement he lived for several years in Connecticut and subsequently in California, spending much of his time in writing. What he wrote had to do largely with exploration, and of this he wrote superbly. Many of his books have become classics and are still widely read all over the world. Indeed, one of Andrews' most important contributions is to be found in the influence of his books upon young people. There can be no doubting the fact that some of our leading paleontologists and zoologists, now in their thirties and forties, were at least in part attracted to their respective fields by reading *Ends of the Earth*, or *On the Trail of Ancient Man*, or *This Amazing Planet*, or *Under a Lucky Star*.

Andrews was the recipient of many honors, including several honorary degrees and medals, and recognition by various scientific societies. His reputation as an explorer was world-wide, and he was much in demand for public lectures.

As a man, Roy Andrews had a cheerful and ebullient personality. His energy seemed to be boundless. He had a supreme zest for life, and he lived life to the full all his days. He died on 11 March 1960, in his 76th year, at Carmel, California, where he had spent the last years of his life. He is survived by his widow, Wilhelmina Christmas Andrews, and by two sons, George Borup Andrews and Roy Kevin Andrews, by his first wife, Yvette Borup.

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Science in the News

Funds for NSF and NASA: Senate Votes To Restore Cuts Made in the House

The Senate has voted to restore the $30 million the House cut out of the National Science Foundation budget, and to not only restore $39 million cut from the space program, but to add another $20 million to the budget request. The bill providing money for the NSF and the National Aeronautics and Space Administration, as well as money for a dozen or so other independent agencies (independent, that is, of the ten cabinet departments) now goes to a House-Senate conference.

The NSF budget is following the steps foreseen by the foundation: a cut in the House, restoration in the Senate, followed by a compromise leaving NSF with all the money (about $180 million) they really expected to get when they set up their budget. The NASA budget is a little more complicated. The Senate Space Committee, chaired by Majority Leader Lyndon Johnson, has been sharply critical of the space budget. Johnson thinks it should be bigger anyway, but he and his committee are more immediately disturbed by what they see as a lack of sophistication on the part of the NASA budget makers.

Honesty or Naivete

They say the space program was delayed this year because NASA failed to allow some leeway for unexpected expenses, particularly money for back-up rockets for firings that did not go off as planned. They said they failed to make allowances, as other agencies do, for the normal cut in the budget to be expected in the House. NASA officials said they were just trying to be perfectly honest with Congress, but the committee seemed to feel there is a point where honesty passes into naivete. Beyond the specific criticisms of NASA's budget-makers the Space Committee applied the general Democratic criticism that the Budget Bureau, rather than the responsible agencies, is making the important decisions; that there is too much thinking about what we can afford to do and not enough about what we need to do.

But the agency has not been completely subservient to the Budget Bureau. The Bureau, it seems, suggested a guideline of $515 million to the agency, but T. Keith Glennan, the NASA administrator, told the committee that "I just couldn't believe it, so we didn't accept it." The eventual estimate reached was $915 million, which is probably just about what the agency will have when the bill gets out of conference. It will very likely get the additional $55 million Johnson's Space Committee authorized as well, but this will come through a supplemental appropriation after the new Congress convenes.

The School Construction Bill: The Victim Is Revived after Being Pronounced Dead Several Times

There may yet be a school construction bill, but it will have to come as the result of some of the most intricate Congressional maneuvering in years. As early as late afternoon of the day the bill reached the House floor it was clear that there was a majority in Congress ready to vote for a compromise bill acceptable to everyone except those opposed to any school bill at all.

Arthur Flemming, the Administration's Secretary of Health, Education, and Welfare, was in the gallery when the bill was being debated in the House. Just after the Powell amendment barring aid to segregated schools had been adopted and the bill, for the moment, appeared headed for defeat, Flemming sent down a suggestion for a compromise move that would have both eliminated the Powell amendment and substituted a bill that the President would be sure to sign. The Democratic leaders, their backs against the wall at this point, were ready to accept Flemming's proposal, but the Republican leader, Charles Halleck, who was against any school bill, blocked the Administration move. The bill then appeared to be killed on a preliminary vote, but was revived and then passed on the formal roll calls that followed.

The bill then went back to the House Rules Committee, which had earlier held up action for 2 months, until it became clear that the school aid forces had the strength and tactical position to bypass the committee. Now two new roadblocks arose: there were two southerners on the Rules Committee unalterably opposed to any school bill. Five other Democrats were for school aid, and a sixth was "persuadable." But together they made up only half the committee, and a majority was needed to send the bill forward to its next step, a House-Senate conference. One of four Republicans on the committee would have to support the bill, and none appeared to be ready to do so. On top of this, Republican leader Halleck was ready to put Clare Hoffman of Michigan, probably the most conservative man in the House, on the House conference committee in place of Peter Frelinghuysen of New Jersey, who had seniority. Together with Graham Barden, of North Carolina, this would make a 2 to 1 majority of the House conferees opposed to any bill. They would combine to prevent the conference from reaching any agreement, so killing the bill.

At this point Vice President Nixon let it be known that he was working on Halleck to get the school bill through. Yet when the Rules Committee met last week no Republican vote was forthcoming and the bill once more appeared to be at death's door. It was an extremely peculiar situation. For Nixon's acknowledgment that he was working on Halleck was a tacit admission that it was the senior House Republicans who were killing this popular legislation which had the support of the Republican Administration. On the eve of his campaign for the Presidency Nixon had laid his prestige on the line on this issue. If the bill got through, Nixon could properly claim some of the credit. But because he had laid his prestige on the line, it was both surprising and damaging that all of the Republicans on the Rules Committee nevertheless voted against the bill.

Some Republicans tried to explain

the situation by claiming that the Republicans were holding out for definite assurances of a compromise that would avoid any possibility of an embarrassing Presidential veto of the final bill. Whether this was true was open to some doubt since there had been earlier reports that a compromise agreeable to the President had already been informally agreed upon. Whatever the true facts, it still remained that if the Republican leadership continued to block the bill it would be a blow to Mr. Nixon. Once the Vice President had publicly involved himself it appeared to be a matter of political necessity that the bill go through, no matter how much Halleck would have preferred to have it blocked.

The Rules Committee met again on Friday of last week and Tuesday of this week without taking any further action on the bill. Yet there appeared to be a very broad feeling among both Democrats and Republicans that the committee would eventually reverse itself and allow the bill to get to conference. Theoretically the Rules Committee could block it again on its way back from conference to the House for final passage, but this was regarded as unthinkable. It looked, after 10 years of frustration, as if there would at last be federal action to alleviate the nationwide shortage of 140,000 classrooms.

Pauling vs. the Senate Internal Security Committee: Contempt Action Likely as He Refuses To Give Names

Linus Pauling, the University of California Nobel-prize-winner, happened to be in Washington last week, and the Senate Internal Security committee took the occasion to call him up to Capitol Hill and ask him to tell about a nuclear test ban petition he circulated in 1958. The committee allowed Pauling to explain his position at length, which he did, often with eloquence.

He said the petition, signed by 11,000 scientists, was initiated entirely by himself and that there were no hidden forces or hidden motives to be uncovered; that he simply thought it a bad idea to continue nuclear weapons testing, which, he said, seems now to be the official view of the United States government. The committee asked him for the names of the people he had written to asking them to help gather signatures, and also the names of those who had responded to his request and sent back lists of signatures. Pauling said he couldn't understand what business it was of the committee to want to know these names.

Legality Denied

He said he would supply the committee with a list of the people he had written to, since that was entirely his own responsibility and implied nothing about the attitudes of the recipients. But he declined to supply the names of scientists who had cooperated in gathering signatures. He said he couldn't see where the committee had any legal right to ask for these names and that he knew from personal experience that giving the names to a Congressional committee could lead to reprisals against the people who cooperated in gathering the signatures who, he said, had been doing nothing more than exercising their constitutional right to petition the government.

The tone of the hearing was fair, even genteel. But at last it took on a familiar ring. The committee issued a formal demand that Pauling return August 9 with the names. Pauling told reporters that he "surely would not" give the committee the names. The committee will, presumably, cite him for contempt. It will then be up to the Justice Department to prosecute the case, and to the courts to decide whether the committee had the right to ask for the names. Recent Supreme Court decisions make the committee's position doubtful.

One step in this procedure may inject the affair into the presidential campaign. A person is cited for contempt of Congress, rather than for contempt of a particular committee. Congress is not likely to be in session to vote on the citation. If it is to take effect it will have to be authorized by the presiding officers of the two houses. In the Senate, of course, the presiding officer is the Vice-President.

Un-American Science

Richard Arens, the staff director of the House Un-American Activities Committee, has told reporters on a number of occasions that he has "enemies" who are constantly working to take his job away from him. The enemies seem to be making some progress.

The source of his difficulties is Arens' extracurricular work for a number of individuals and organizations of the sort that even William F. Buckley has called psychopathic. One of the most curious, and the one which seems to have gotten Arens into the deepest trouble, is a committee, consisting mainly of Arens himself, which is helping a New York millionaire named Wycliffe Draper to give away grants for research in "genetics and immigration." What Draper would like is some good solid scientific evidence that Negroes are congenitally inferior to whites, and a nice, wholesome, workable plan to "immigrate" them back to Africa. Draper apparently felt that the responses his offers of funds were getting were an insult to his dignity. He seems to have engaged Arens to avoid any more such unpleasantness, for Arens was to provide him with the names of people who could be expected to accept Mr. Draper's generous offer in support of science. All of this was brought to the attention of Francis Walters, chairman of the Un-American Activities Committee, who reportedly did not seem much upset, and to Speaker of the House Sam Rayburn, who appeared extremely upset. If Rayburn has his way, which he generally does, it is believed that Arens will no longer be staff director when the next Congress convenes.

U.S. Support for Foreign Research

The House has passed its version of a bill to allow the use of foreign currencies for grants and fellowships to promote medical research abroad. The program will use money accumulated through the sale of food surpluses. The bill now goes to conference with a broader bill already passed by the Senate, which allows spending of $50 million a year for this purpose.

This will be the newest of a number of programs authorizing the use for scientific and educational purposes of foreign currencies accumulated by the U.S. government. The Fulbright scholarships, of course, have long been financed in this way. Another program, again using funds obtained through the sale of food surpluses, allows the Department of Agriculture to make research grants to foreign institutions. Last week, for example, the Department announced seven grants to Polish institutions for studies of plant genetics, livestock parasites, and forestry. The total for these agricultural grants has been running to about $1.6 million a year, and is expected to increase.—H.M.

News Notes

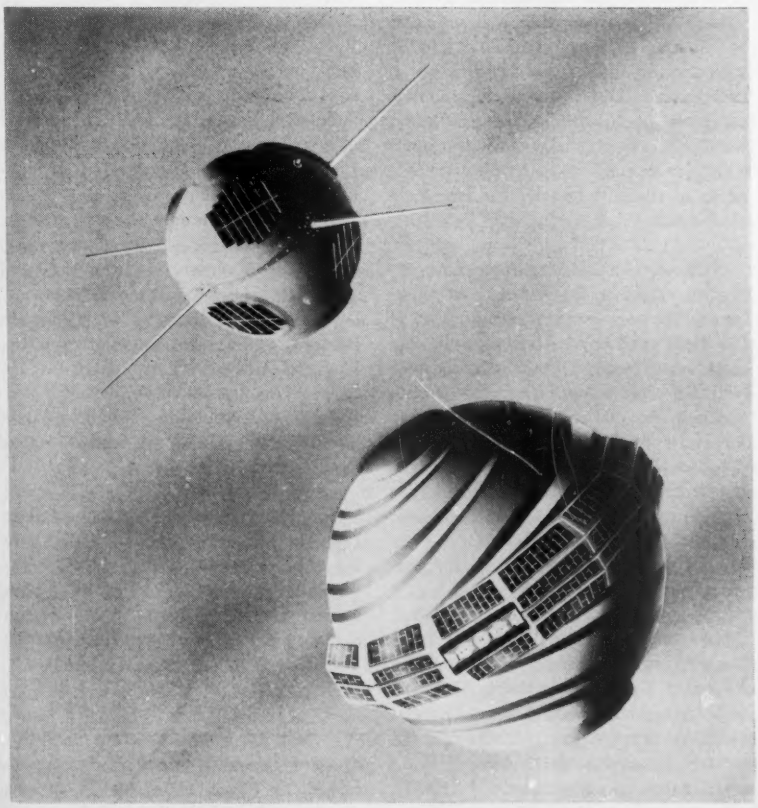
New Satellite Working "Fantastically Well," Says the Navy

The Transit II-A satellite that the Navy fired into orbit last week can be used to define a ship's position to within a tenth of a mile, about one fifth the margin of error of conventional stellar navigation methods. The new Transit is traveling in an orbit ranging from 382 to 657 miles above the earth, accompanied by the "Grebi," a smaller satellite for measuring solar radiation which was fired into orbit by the same launching rocket.

Navy officials described themselves as astounded by the accuracy of the device. They said that the experiment puts the satellite navigation program well ahead of schedule. They expect that the next launching, scheduled for the fall, will allow the limited use of the system by commercial ships. The Navy says it now expects to have a fully operational system installed sometime in 1961. This will probably involve four Transits, enough to have one of the navigational satellites pass within range of a ship's receiving equipment every 15 minutes.

The high accuracy of this system will be used to provide extremely precise information on the shape of the earth, derived from the slight changes in orbit which can be detected by using the Transit instrumentation. The key to the system lies in making use of calculations based on the Doppler effect, that is, on the slight changes in signal frequency as the satellite approaches and passes an observing station. The great value to shipping of the system is that ships will no longer have to rely on crude calculations when bad weather makes solar or stellar navigation impossible.

The technique of launching two satellites with a single rocket will probably be used often in the future. It was regarded as having a number of technical advantages over putting all instrumentation for a number of independent experiments into one satellite.



The "Greb," a 42-pound satellite for measuring solar radiation, is orbiting the earth slightly ahead of the Transit-2A navigational satellite.

Expedition to the Indian Ocean

A major oceanographic undertaking, the International Indian Ocean Expedition, will get under way late this year and will continue through 1964. It will greatly extend man's knowledge of these least-known waters of the world, which cover a seventh of the earth's surface. Like the recent International Geophysical Year, the expedition will constitute a many-sided scientific attack on a single area of interest under the leadership of a special committee of the International Council of Scientific Unions, a nongovernmental organization with headquarters in The Hague.

Scientific responsibility for United States participation will be borne by the National Academy of Sciences–National Research Council. The U.S. Navy will make available oceanographic ships sponsored by the Navy and operated by leading United States oceanographic institutions. The National Science Foundation will be responsible for planning and coordinating federal support for U.S. participation in the program, including financial support.

The expedition's peak activity is expected to occur during 1962 and 1963 when ships and scientific personnel from well over a dozen nations will be conducting basic research in physical and chemical oceanography, meteorology, marine biology, geophysics, and submarine geology.

Physicists Form Commission on College Physics Teaching

Formation of a nationwide commission to plan a coordinated national program for the teaching of college physics was completed by 60 physicists representing 36 colleges and universities, industry, and various professional organizations during a recent 3-day conference at the University of Minnesota. The commission, to consist of 17 members, including two ex-officio members from the American Association of Physics Teachers and one from the American Institute of Physics, will begin planning its program at once. The conference was the third such meeting sponsored by the American Association of Physics Teachers and supported by the National Science Foundation.

The physicists proposed that the commission, tentatively named the Commission on College Physics, be associated with the American Association

of Physics Teachers and be responsible to it, but that it be authorized to plan its own program, to seek financial support, and to employ necessary staff and consultants.

The commission will call upon the entire physics profession to help in the preparation of teaching aids, including books, laboratory apparatus, demonstration equipment, and films, and will organize investigations into such problems as curricula, teacher training, and supply and apparatus development and distribution. Universities and colleges will be invited to participate in the collaborative effort and to support participation of their staff members in the program. Support funds will be sought from governmental sources and from private foundations.

Specific phases of the program will be planned and carried out by individual physicists, by the professional associations of physicists, and by the commission and its various subcommittees. It is expected that several hundred physicists will be participating in the program when it reaches its full level of activity.

British Association To Meet in Cardiff

The British Association for the Advancement of Science will hold its 122nd annual meeting in Cardiff, 31 August–7 September. The presidential address of Sir George Thomson, "The Two Aspects of Science," will highlight the week's events.

Participants in a full-day symposium on the world food and population situation will outline the major problems and discuss immediate steps that can be taken.

The Kelvin, Darwin, and Lister lectures, which were inaugurated at the 121st meeting to encourage young scientists to explain the significance of their work in nontechnical language, will be a major attraction of the sectional programs.

An extensive program of excursions, including visits to industrial and commercial establishments in South Wales and Monmouthshire, is planned.

News Briefs

Forestry congress. The 5th World Forestry Congress will meet on the campus of the University of Washington, Seattle, 29 August–16 September. The congress, which will be attended by more than 2000 foresters from 50 nations, will give special consideration to the problems of multiple use of forest lands. Papers will be presented on silviculture and management, genetics, protection, economics and policy, education, products, forest and range watersheds, recreation and wild-life, operations, and tropical forestry.

* * *

Physics meeting. The first official reports of the world's most powerful accelerator, the 25 billion electron volt machine at the European Center for Nuclear Research, will highlight the 10th international Conference on High Energy Physics at the University of Rochester, 25 August–1 September. The conference, sponsored by the International Union of Pure and Applied Physics and other organizations, will be attended by 325 scientists from 31 countries. Membership is by invitation, and arrangements are being made by a commission of six, directed by Robert E. Marshak, chairman of the department of physics and astronomy at the University of Rochester.

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Russian studies center. The University of Illinois has established a Center for Russian Language and Area Studies with the support of the U.S. Office of Education. Ralph T. Fisher, associate professor of history at the university, will direct the center. The programs will give special consideration to the needs of students in scientific and technical fields.

* * *

Natural resources group. The appointment of a 29-member Advisory Committee on Natural Resources of the Democratic Advisory Council has been announced by the Democratic National Committee. The new group, which is headed by former Assistant Secretary of the Interior C. Girard Davidson of Oregon, will prepare a policy statement on natural resources problems for the Democratic party and will also prepare planks on natural resources for the Democratic platform in the coming campaign.

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New congress. The 1st international Congress of Histochemistry and Cytochemistry will meet in Paris from 28 August to 3 September. The congress, which is being organized through the Société Française d'Histochimie and other national histochemical societies, will demonstrate the practical applications of recent research. The program includes sections on physical problems, biochemical applications to histochemistry, and applied histochemistry. Inquiries should be sent to R. Wegmann, Secretary General, 45, Rue des Saints-Pères, Paris, 6e.

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Radiation data. The U.N. Scientific Committee on the Effects of Atomic Radiation, established by the General Assembly in 1955, wants to receive any data relevant to radiation research. The committee does not carry on research but depends on the work of scientists in the member states for the material which it studies and collates. Scientists who are interested in contributing information on physical or biological topics should send four copies of their reports to Edward R. Gardner, Director, Office of Special Projects, U.S. Atomic Energy Commission, Washington 25, D.C.

Scientists in the News

The following scientists received the first Ernest Orlando Lawrence Memorial Award of the Atomic Energy Commission on 27 June. The award consisted of a medal, a citation, and $5000 for each recipient.

Harvey Brooks, dean of engineering and applied physics, Harvard University, for his work on reactors.

John S. Foster, Jr., associate director of the Lawrence Radiation Laboratory, Livermore, Calif., for work in developing atomic weapons.

Isadore Perlman, associate director of the Lawrence Radiation Laboratory, for the isolation of plutonium and transplutonic elements.

Norman F. Ramsey, Jr., associate professor and professor of physics, Harvard University, for contributions to experimental nuclear physics.

Alvin M. Weinberg, Oak Ridge National Laboratory, for contributions to nuclear reactor theory.

Victor H. Baptist, of Don Baxter Laboratories, Glendale, Calif., and **Otto E. Lobstein**, of Chem-Tech Laboratories, Beverly Hills, Calif., have been named visiting research professors in the department of chemistry of the University of Redlands, Redlands, Calif.

Robert L. Folk, associate professor of geology, University of Texas, received the President's Award of the American Association of Petroleum Geologists, which is given each year to the

author, or authors, below the age of 35, whose article in the association's *Bulletin* of the preceding year is considered the most significant contribution to petroleum geology. Folk's award-winning article, "Practical Petrographic Classification of Limestones," appeared in the January 1959 issue.

Adriance S. Foster, chairman of the department of botany at the University of California (Berkeley), has been elected to honorary membership in the Zoologisch-Botanische Gesellschaft in Vienna for his studies in plant anatomy and related fields.

Harry Hookway, assistant director of the British National Chemical Laboratory, has been named director of the United Kingdom Scientific Mission in Washington to succeed **E. S. Hiscocks**, who will return to Britain in the fall to become director of the Tropical Products Institute. Hookway is known for his work on polymers, ion-exchange resins, and saline water conversion.

Weikko A. Heiskanen, director of the Institute of Geodesy, Photogrammetry, and Cartography at Ohio State University, received an honorary degree from Uppsala University, Uppsala, Sweden, on 31 May.

R. C. Fuller, scientist in the biology department of the Brookhaven National Laboratory, has been named professor of microbiology and chairman of the department at Dartmouth Medical School. He is currently on leave as a senior postdoctoral fellow in the department of biochemistry at the University of Oxford.

J. W. Foster, professor of bacteriology at the University of Texas, is making a survey of microbiology in Japan this summer. His visit is sponsored by the Microbial Chemistry Research Foundation of Japan and will include lectures at universities and visits to institute and industrial microbiology laboratories.

Peter King, associate director of research for materials of the Naval Research Laboratory, has been awarded the Distinguished Civilian Service Award for his work in developing the long-range detection program which enabled the United States to discover the first atomic explosion by a foreign power, in 1949.

James A. Miller, professor of anatomy at Emory University, has accepted the chairmanship of the department of anatomy at Tulane University.

Edward S. Knipling, director of the entomology research division of the Agricultural Research Service, Beltsville, Md., and **Raymond C. Bushland**, head of the service's research program on insects affecting livestock, at Kerrville, Tex., received the $10,000 Hoblitzelle National Award in the agricultural sciences on 18 May. The entomologists were selected by the Texas Research Foundation for their development of the "male-sterile" technique to eliminate the screwworm fly which infests cattle.

Homer E. Newell, assistant director of space sciences at the National Aeronautics and Space Administration, is now deputy director of space flight programs.

Joseph B. Casagrande, staff member of the Social Science Research Council, New York, has been appointed professor of anthropology and head of the department at the University of Illinois.

Kenton L. Chambers, assistant professor in the department of botany at Yale University, has been appointed associate professor and curator of the herbarium at Oregon State College.

I. I. Rabi, Higgins professor of physics at Columbia University and winner of the 1944 Nobel Prize in physics, received the 1960 Barnard Medal of the university at Columbia's 206th commencement. Rabi was cited as the "principal author of a revolution in experimental and theoretical physics."

Georg and **Eva Klein**, of the Karolinska Institutet in Stockholm, have been selected to receive the third annual Bertha Goldblatt Teplitz Award by the Ann Langer Cancer Research Foundation of Chicago. Georg Klein, who is professor and head of the Institute for Tumor Biology of the Karolinska Institutet Medical School, and his wife, who is associate professor of medical cell research, will each receive $500.

Theodore Delevoryas, assistant professor of botany at Yale University, has been named associate professor of botany at the University of Illinois.

The Association of Universities for Research in Astronomy, Inc. (AURA), has named **Nicholas U. Mayall**, astronomer at Lick Observatory, as director of the Kitt Peak National Observatory. He succeeds **C. D. Shane**, president of AURA and astronomer at Lick Observatory, who has been acting director since the resignation of Aden B. Meinel in March.

Recent Deaths

James T. Case, Santa Barbara, Calif.; 78; professor of radiology at Northwestern Medical School from 1912 to 1947; director of the Memorial Cancer Foundation, Santa Barbara; fellow of the Royal Society of Medicine and former president of the American College of Radiology; 24 May.

Donald S. Childs, Syracuse, N.Y.; 72; professor emeritus of radiology at Syracuse University; attending roentgenologist at St. Joseph's Hospital; secretary-treasurer of the Radiological Society of North America; 27 Apr.

John Elmendorf, Baltimore, Md.; 67; staff member of the division of public health of the Rockefeller Foundation, 1920–53; former director of the National School of Hygiene of Colombia; expert on malaria; 27 May.

John F. Fulton, Hamden, Conn.; 60; Sterling professor of the history of medicine at the School of Medicine of Yale University; neurophysiologist and former chairman of the department of physiology at Yale; developed aeromedical research at the Yale Medical School; 29 May.

Harold E. Jones, Paris, France; 65; director of the Institute of Human Development and professor of psychology at the University of California, Berkeley, since 1927; pioneer in child development studies; 7 June.

Dudley J. Morton, New York, N.Y.; 76; former associate professor of anatomy at Columbia University and research associate of the American Museum of Natural History; orthopedic surgeon; 22 May.

Howard T. Orville, Baltimore, Md.; 58; chairman of the Advisory Committee on Weather Control, 1953–58; pioneer in the use of high-altitude balloons in meteorology; 24 May.

Joseph P. Weinmann, Chicago, Ill.; 64; professor of pathology in the College of Medicine and head of the Division of Oral Pathology in the College of Dentistry of the University of Illinois; 15 May.

Book Reviews

The Strategy of Conflict. Thomas C. Schelling. Harvard University Press, Cambridge, Mass., 1960. x + 309 pp. $6.25.

Strategy of Conflict is an extraordinary book; it sheds completely new light on the inner mechanism of human conflicts as they develop and finally reach their conclusion.

One of Schelling's central themes is the decisive terminal stage of conflict, the "moment of truth" when the will of one opponent prevails. At that moment, it is no longer strength or skill that counts. What the participants could do to each other by exploiting their positional advantages, they have done. But all this, Schelling argues, does not decide the outcome, or at least not completely. In many interesting cases—in bargaining, for example, which is analyzed at length—the participants go into the terminal stage with an indeterminate problem on their hands. Their positions are still apart, but each would rather settle on the opponent's terms than to fail to make a deal. If both remain stubborn, then both will get something worse than they could have achieved. Then what? They could throw dice, or split the difference (this happens often enough), but there is also a third possibility. By using "bargaining strategy," one of the opponents can force the other to settle on *his* terms.

What is bargaining strategy? It is the art of generating "bargaining power" which, according to the author, consists in "the power to bind oneself." In order to force *B* to settle on *A*'s terms, *A* must prove to *B* that the matter is out of his (*A*'s) hands—that he could not abate from his terms even if he wanted to. One will win, that is, *impose* a choice upon the other, by divesting *oneself* of the freedom to choose, while leaving the *other's* freedom unimpaired. This paradoxical essence of bargaining strategy is illuminated in many fascinating variants.

The same paradox, however, is also involved in other forms of conflict than bargaining; in fact, this paradox is, for Schelling, the essential feature of *all* strategy. Let us take deterrence, for example. In order to deter another from doing something objectionable, one must make a credible retaliatory threat. The technique is trivial *if* the recipient of the threat has no doubt that the threatener is perfectly capable of punishing him without being hurt himself. Deterrence then is not a matter of "strategy," and there is no "conflict" worth speaking of—one is master, that's all. But what if the recipient of the threat has a reason to suppose that the threatener, too, would be hurt in carrying out his threat? Then there is genuine conflict, one that involves two parties opposing, threatening, and possibly fighting and hurting each other; this gives rise to real problems of strategy. For example, if it is understood by both parties that "no challenge, no retaliation" would be better for each than challenge and retaliation, the deterrer can make his will prevail by applying the strategy of proving to the would-be challenger that once a challenge is made, carrying out or not carrying out his threat is no longer a matter of free choice for him. If, in these circumstances, the would-be challenger retains his freedom of choice, he will not strike (he will be deterred). If the challenger, too, applies the strategy of binding himself, both will lose.

Why does one opponent's divesting himself of his freedom of choice "win" the game for him? It is because underneath the clash of interests there is also mutual dependence. Not all possible outcomes are just good for one party and bad for the other, or else neither good nor bad for either. Some possible outcomes are bad for *both*, while some others are *mutually* preferable to these. In other words, in bargaining, in deterrence, and in many other comparable conflict situations, the game is nonzero-sum with a peculiar feature: the players can avoid some possible outcomes that would be bad for both, and achieve others that are relatively better for each, without being good for both.

By introducing this type of nonzero-sum game in which the players' motives are mixed—partly antagonistic, partly nonantagonistic—Schelling extends the scope of game theory. The "classic" game theory of von Neumann and Morgenstern, he argues, is essentially a theory of zero-sum games and thus applicable to *pure* conflict, in which players act *only* from antagonistic motives. Against this, Schelling proposes a new type of theory, applicable to mixed-motivation games, a theory in which "pure" conflict (the zero-sum game) appears only as a limit case, and which encompasses, in addition to this, *two* types of nonzero-sum game: the mixed-motivation game just discussed, and the game of "pure cooperation" in which all antagonism vanishes and all possible outcomes are as good or better for both players than the results they could attain without cooperation.

In this new type of game theory, much of the mathematical scaffolding of the "classic" theory disappears and is replaced by concrete, nonformalized, nonformalizable decision-principles. Also, all basic concepts of the theory are redefined. In the Schelling type of game theory, for example, "strategy" means something radically different from "strategy" in the von Neumann-Morgenstern sense. To be sure, both concepts of "strategy" refer to lines of action chosen by each player in view of what he expects his opponent to do. But the "classic" concept of strategy involves no influence directly exerted by the will of one player upon the will of the other, whereas the point of "strategy" in the Schelling sense is just this. Finally, while "classic" game theory can be developed with pencil-and-paper methods, the Schelling type of theory is in part empirical, calling for experimental investigation. Some of the most valuable material presented in the book has to do with the experimental study of nonzero-sum strategies.

Several highly important political and strategic problems now on the agenda (notably surprise attack, disarmament, and limited war) receive illuminating treatment in terms of the new theory. I cannot attempt here to demonstrate in detail either the fruitfulness or the limitations of Schelling's approach. It is enough to stress the extraordinary theoretical and practical importance of his central idea that conflict situations typically involve some element of mutual

dependence mixed with antagonism.

In fact, it seems to me that Schelling himself has not grasped the full significance of his approach. Looking at the matter closely, we discover that the implications of his conflict analyses are even more radical than he himself allows. "Pure" zero-sum conflict does not represent an extreme type of institutionalized conflict. It can be shown that it cannot exist in a stable, institutionalized form.

Let us consider, for example, zero-sum parlor games like chess. The *formal* relationship between the players in their role as players is zero-sum. Their *existential* relationship, however, is not: as existing human beings, they are not merely locked in conflict; they also derive mutual pleasure from playing the game, and that is why they are playing it. Zero-sum game interaction is embedded here in a cooperative existential relationship. In all such cases, there is mutual interest in maintaining the integrity of the game, in playing according to the rules. Cheating in parlor games, on the other hand, introduces another nonzero-sum element—mutual damage—since it tends to undermine motivations to play the game and thus to eliminate the game as a source of profit to the cheater.

Schelling indicates a way of transforming chess into a nonzero-sum game by offering rewards for pieces that remain on the board. True, if we do this, there will be mixed game motivations; winning will not be the only thing that counts. But from the existential point of view, the game is nonzero-sum anyway. There *must* be another motivation than "winning," if the game is to remain alive as an institution.

What about sharp existential conflict situations such as duels and wars? One could argue that duels to the death, disregarding the unlikely outcome of both duelers being killed, represent an institutionalized form of zero-sum conflict. This would indeed be the case if the participants regarded being killed as the worst possible outcome. But in cultures where being killed is considered the worst that can happen to someone, such duels will disappear as an institution. The institution of mortal combat can subsist only where the dishonor of avoiding the risk of being killed is mutually deemed worse than death. Then the duelers will satisfy a mutual need for honor by fighting each other to the death. This mutual need alone can sustain the institution.

As to zero-sum war, the question is, to begin with, whether things can be so arranged institutionally that no mutually damaging outcomes *can* occur. This condition could be satisfied only by two types of war. One is a limited war with a stop rule such that the war must end when one side is clearly ahead or when there is a stalemate with both sides having lost their expendable forces. The other is a war in which one side is so much stronger that it does not face the risk of being worse off as a result of having fought. Both types of war have zero-sum features. The former, however, presupposes drastic limitation and, hence, the recognition of mutual dependence; it is somewhat like the zero-sum parlor game—a sharp conflict embedded in a more cooperative existential relationship. As to the latter, it presents a trivial strategic problem and will tend to drop out of the institutional picture: where A is so much stronger, B is far more likely to recognize him as master than to fight him. In all other types of war, mutual loss is a priori possible, and the game is nonzero-sum.

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Le Razze e i Popoli della Terra. vols. 1–4. Renato Biasutti *et al.* Union Tipografico-Editrice, Turin, Italy, ed. 3, 1959. 2914 pp. Illus. L. 37,000 (approximately $60).

The English language does not contain, as far as I know, a complete, modern work on global anthropology. The Italian language does. It is Renato Biasutti's mammoth, four-volume work on the races and peoples of the world, which was revised for the second time in 1959. According to my bathroom scales the volumes weigh 22 pounds, thus, its price is $2.73 a pound, less than twice the cost of good beefsteak. In the library of one American university, students who have not studied Italian keep the volumes in constant use, copying the numerous folding maps showing the distributions of boat types, house types, and other phenomena rarely charted in English-language publications. The type page is 8 by 5½ inches, and there is an illustration on two out of every three pages, as well as 45 tables in color (15 of which are pictures) and 30 maps, mostly folding. Were this book in English it would fill a crying need, but it is unlikely to be translated because of the cost.

The volumes cover human evolution, racial history, prehistoric archeology, racial movements and distribution, linguistics, and ethnography. To help him write it, the geographer, Biasutti, whose university post is at Florence, and who is now 82, enlisted the aid of 17 other professors—M. Bartoli, R. Battaglia, E. Cerulli, L. Cipriani, R. Corso, G. Genna, G. Gentili, P. Graziosi, L. Grotanelli, J. Imbelloni (Argentina), A. Micheli, M. Muccioli, N. Puccioni, S. Sergi, C. Tagliavini, T. Tentori, and G. Vidossi. All except Graziosi and Tentori contributed signed chapters either singly, in collaboration with Biasutti, or in collaboration with each other. Next to the maestro's, Battaglia's name appears most frequently. In addition to writing one section, Cipriani, who is famous for his photography, contributed hundreds of magnificient photographs.

Without great elaboration, little more can be said about this publication except that it is written in a uniformly simple style, so that anyone with the rudiments of Italian (or even just French or Spanish) can use it; that it is up to date; and that it is mostly noncontroversial. Biasutti's classifications of races and culture are based on geography, evolutionary status, and history. The concepts of several other schools of anthropological thought are explained, and the coverage is monumental. Although its function is encyclopedic, this opus is a much better teaching device than an encyclopedia. While too expensive for use as a textbook in any language, it belongs in every anthropological library. Many a junior professor of sociology and anthropology or some other combined field, faced with working up a course in general anthropology, will find it a godsend.

CARLETON S. COON
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Men and Moments in the History of Science. Herbert M. Evans, Ed. University of Washington Press, Seattle, 1959. viii + 226 pp. Illus. $4.50.

The occasion for publishing this collection of nine essays was the 25th birthday of the History of Science Dinner Club, founded by Herbert Evans in 1932. The first essay, by Egon Brunswick, is a survey of "ontogenetic and other developmental paral-

lels in the history of science." It describes Brunswick's own work on the development of criticism in children and J. Piaget's parallels between theorizing in children and in the early history of physics. The stages of egocentrism, functionalism, and overgeneralization are recognizable in some of the stories of historical events which follow: Robert H. Lowie speaks of ethnocentrism as a stage in the development of ethnography; J. B. Stallo's critique of classical physics, explained here by Stillman Drake, can be understood as a fight against overgeneralization; and Newton's *Hypotheses Non Fingo*, to which E. W. Strong here devotes a penetrating study, could be linked to the search for the *genus proximum*, according to Egon Brunswick.

E. O. Essig gives a sympathetic picture of an almost unknown hero of science, Charles Fuller Baker, whose insect collection of about a quarter of a million specimens was saved by the Smithsonian Institution.

In the "Essays in biology," the *Festschrift* honoring Evans on his 60th birthday (1942), Frederick O. Koenig dealt with Sadi Carnot's thermodynamic theorems; he extends this study here to a detailed history of the second law of thermodynamics. Victor F. Lenzen presents a somewhat dry account of Max Planck's philosophy of science. Leonardo Olschki shows the wide influence that radiated from Marco Polo's description of the world. The last essay, on the first determination of stellar parallax, recreates the dramatic events of 1837–39 and the part played by Wilhelm Struve; the author is his great-grandson, Otto Struve.

This is a book for those adventurous spirits who love to make excursions beyond their fields of specialization.

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Plant Pathology. An advanced treatise. vol. 2, *The Pathogen*. J. G. Horsfall and A. E. Dimond, Eds. Academic Press, New York, 1960. xiv + 715 pp. Illus. $22.

In the second volume of their trilogy on plant pathology, the editors and their collaborating authors maintain the high standards established in volume 1. The theme of the present volume is the pathogen, in contrast to that of the earlier volume, which was centered around the diseased plant. In an interesting introductory paragraph, careful, even forceful, distinction is drawn between parasites and pathogens and, necessarily, between the resulting phenomena of parasitism and pathogenism. Many readers will be surprised to learn that these terms are not synonymous. The authors also emphasize that, in their opinion, diseases are *caused*, not *incited*, although the latter term has become increasingly popular in recent years. Pathogens are of many kinds, including not only the fungi and bacteria (which are usually thought of) but such diverse agents as nematodes, mites, insects, viruses, and many inanimate entities (for example, chemical deficiencies or excesses, and even various phases of unfavorable weather).

In the single chapter devoted to parasitism, George L. McNew thoroughly reviews the subject, presenting his material under such topics as the nature, origin, evolution, and physiology of parasitism. His discussion of the law of host-parasite balance in pathogens is particularly effective. In contrast, the remaining 13 chapters, each written by a highly qualified specialist, are devoted to pathogenicity or the ability of the parasite to produce disease. Three general phases of the subject, reproduction of the pathogen, the nature of pathogenicity, and the mechanisms of inhibiting the pathogen, are considered.

F. C. Bawden reviews the multiplication of viruses, broadening his presentation by including such topics as the differences between viruses and organisms, and analogies with bacteriophages. Lilian Hawker discusses the reproduction of bacteria, actinomycetes, and fungi. The insects and arachnids are left to the entomologists. Spore germination and the various factors affecting the phenomenon are discussed by V. W. Cochrane.

The broad field of the nature of pathogenicity or the ability of the organism to cause disease is presented in six chapters. Major topics considered are the mechanical and chemical ability to break host barriers; interactions of pathogen, soil, soil microorganisms, and host; the genetics of pathogens; and toxins. The problem of finding mechanisms to inhibit pathogens is met by a careful review of the current knowledge of virus inactivation and of the physiology and chemistry of fungicides.

The nematodes come into their own with the concluding chapter which, although headed nematocides, covers a broader field and is, in effect, a brief but thorough account of plant diseases caused by nematodes.

As in the previous volume the indexes are extensive and adequate.

JOHN A. STEVENSON
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Infectious Diseases of Animals. vol. 1 and vol. 2, *Diseases Due to Bacteria*. A. W. Stableforth and I. A. Galloway, Eds. Academic Press, New York; Butterworths, London, 1959. 396 pp.; 414 pp. Illus. $18 each; 2 vols., $33.

The first two volumes of a proposed encyclopedic record of the infectious diseases of animals have now been published. The subject matter in these two volumes is limited to the diseases caused by bacteria; diseases caused by rickettsia, viruses, and protozoa are to be covered in later volumes of the series. The editors have assembled an imposing group of British authorities, and each member of the group has written in a field of his special interest. In spite of the plethora of authors, the two volumes have exceptional continuity and uniformity.

The first volume contains chapters on actinomycosis and actinobacillosis, anthrax, brucellosis, clostridial diseases, coliform diseases, corynebacterial diseases, fungal diseases, glanders and melioidosis, Johne's disease, leptospirosis, and listeriosis. The second volume covers necrobacillosis, pasteurellosis, the pleuropneumonia group of diseases, swine erysipelas, tuberculosis, and vibrosis. The chapters are arranged alphabetically, a valuable point for the student.

Each causative agent is described in thorough detail, and in most instances the epidemiological and clinical features are adequately covered. The gross lesions are usually listed and described in some detail under "pathology," but rarely are microscopic lesions described. A few photomicrographs are used, but most of these are not of good quality. The other illustrations, particularly the charts, tables, and line drawings, although used sparingly, are informative and of good quality. The type is easily readable, and the paper is excellent.

The authors obviously made good use of the literature, particularly that

published in Great Britain and North America, and a list of references is found at the end of each chapter. In general, Bergey's *Manual of Determinative Bacteriology* is followed for the classification and nomenclature of bacteria. In most cases use is made of the currently favored name of an organism (for example, *Fusiformis necrophorus*). No mention is made of the many other names which have at one time or another been acceptable for the same organism. The inclusion of some of these older names (even parenthetically) might have been helpful in many instances.

In general, these two volumes are complete and authoritative, and each should be in the library of everyone who is seriously interested in animal disease.

T. C. JONES

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Introduction to Statistical Communication Theory. David Middleton. McGraw-Hill, New York, 1960. 1140 pp. Illus. $25.

At first glance, this is a very impressive volume on a subject which is not adequately treated in textbook form. However, its very size raises the question of the purpose of this book. Surely only a massive course of lectures would require even a quarter of the material contained in this book. For this reason it might appear to be an imposition on the students to require that they purchase a text at this price; however, there are other objections to the use of this volume as a text. Matters of price aside, I find Middleton's book to be quite diffuse and without emphasis on what is, and what is not, important knowledge of the statistical foundations of communication systems analysis. In addition, the notation is often cumbersome and distracting.

As far as subject matter is concerned, the table of contents omits nothing that is relevant to the field; there are chapters on statistical ensembles, spectra and correlation functions, sampling and interpolation, information theory, Gaussian processes, Langevin and Fokker-Plauck equations, thermal noise, rectification of amplitude-modulated waves, optimum filtering, and finally, a large section on decision procedures applied to reception systems. However, length does not necessarily imply complete coverage of all of these subjects. For example, the relatively simple proof that a one-dimensional, stationary Gaussian process will be Markoffian if, and only if, the autocorrelation function is an exponential is left as a problem. The same is true in the case of many other important results. Random walks are discussed, but only enough to whet the reader's appetite. The simpler and shorter discussion of random walks which is given in Feller's book is more illuminating regarding the transition from the discrete random walk to the diffusion equation.

Although the preceding remarks might be regarded as sanguine, I find Middleton's book to be eminently suitable as a reference work for those who are familiar with communication theory. It is a very nearly complete summary of our present knowledge (although some important topics such as the Siegert-Darling theory of linear functionals of random signals are only mentioned) together with a more than adequate bibliography. If the reader can supply his own emphasis, this is a valuable compendium on communication theory.

GEORGE WEISS

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New Books

General

Gubnitz, Myron B. *Rocketship X-15*. Messner, New York, 1960. 288 pp. $4.95. Story of the experimental aircraft designed to take man higher and faster than ever before.

Harwell, George C. *Technical Communication*. Macmillan, New York, 1960. 342 pp. $3.75. A textbook that is primarily intended for the engineering student; chapters 5, 6, and 7 discuss formal and informal reports and technical articles.

Jacobs, Jake. *Marineland Diver*. Dodd, Mead, New York, 1960. 190 pp. $4.

Johnson, Walter. *1600 Pennsylvania Avenue*. Presidents and the people, 1929–1959. Little, Brown, Boston, 1960. 400 pp. $6.

McGlothlin, William J. *Patterns of Professional Education*. Putnam's, New York, 1960, 316 pp. $6.75.

Marder, Daniel. *The Craft of Technical Writing*. Macmillan, New York, 1960. 414 pp. $5. A discussion of the principles of rhetoric basic to the writing situations encountered by scientists and technical men.

Thomson, Charles A. H., and Frances M. Shattuck. *The 1956 Presidential Campaign*. Brookings Institution, Washington, D.C., 1960. 397 pp. $5.

Mathematics and Physical Sciences

Ajzenberg-Selove, Fay, Ed. *Nuclear Spectroscopy*. Parts A and B. Academic Press, New York, 1960. 1184 pp. Parts A and B, $16 each; part B, prepublication price $14 until 31 August.

Emeléus, H. J., and J. S. Anderson, Eds. *Modern Aspects of Inorganic Chemistry*. Van Nostrand, Princeton, N.J., ed. 3, 1960. 622 pp. $7.75.

Emmett, Paul, H., Ed. *Catalysis*. Oxidation, hydration, dehydration, and cracking catalysts. vol. 7. Reinhold, New York; Chapman and Hall, London, 1960. 383 pp. $13.50. This volume completes the present series on catalysis. The contributors include J. K. Dixon, J. E. Longfield, L. B. Ryland, M. W. Tamele, J. N. Wilson, and M. E. Winfield.

Jacobs, Alan M., Donald E. Kline, Forrest J. Remick. *Basic Principles of Nuclear Science and Reactors*. Van Nostrand, Princeton, N.J., 1960. 270 pp. $6.50. An introduction to the design and use of nuclear reactors and radioisotopes.

Jacobs, Horace, Ed. *Advances in the Astronautical Sciences*. vol. 5. Plenum Press, New York, 1960. 364 pp. $8. This volume, the proceedings of the 2nd western national meeting of the American Astronautical Society (held 4–5 August 1959 in Los Angeles, Calif.), contains 26 reports covering space mechanics, control, and guidance; advanced propulsion and power; astronautical systems and space vehicle design; space communication and instrumentation; and lunar and planetary environment.

Karan, Pradyumna P. *Nepal. A Cultural and Physical Geography*. Univ. of Kentucky Press, Lexington, 1960. 104 pp. $10.

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Reports

Suppression of Pain by Sound

Abstract. A procedure involving music and noise has been effective in suppressing pain in 5000 dental operations. The music promotes relaxation, and the noise (the main agent) directly suppresses pain. The dental procedure and results are described, and an explanatory hypothesis is suggested.

Certain types of pain may be reduced or abolished by intense acoustic stimulation (*1*). We have studied "audio analgesia" in dental situations and, with others, have obtained preliminary results in hospitals and laboratory (*2*).

In Wallace J. Gardner's dental office, suppression of pain by sound has been fully effective for 65 percent of 1000 patients who previously required nitrous oxide or a local anesthetic in comparable operations. For 25 percent, sound-induced analgesia was sufficiently effective that no other analgesic or anesthetic agent was required. For 10 percent, it was less than adequate. In only a handful of cases has a patient reported experiencing objectionable pain while listening to the intense sound.

During the last year, audio analgesia equipments have been used by eight other dentists in the Boston area. Their experiences have paralleled those just summarized. In about 90 percent of 5000 operations, sound stimulation has been the only analgesic agent required. Gardner has extracted over 200 teeth without encountering any difficulty or report of objectionable pain. The other dentists, also, have extracted teeth under audio analgesia.

The procedure usually followed in inducing the analgesic condition involves the use of music and of noise. The patient wears headphones and controls the stimuli through a small control box in his hand. Before the operation, and until a potentially painful procedure has to be employed, the patient listens to stereophonic music. As soon as he anticipates pain or feels incipient pain, he turns up the intensity of the noise stimulus. It is random noise with a spectrum shaped by low-pass filters to provide a compromise between analgesic effectiveness and pleasantness of quality.

The main function of the music is to relax the patient. For most patients, the noise is the main agent, the one that drowns out the pain.

Several factors operate simultaneously in producing the analgesia (*3*). The noise appears, in introspection, directly to suppress the pain caused by the dental operation. During cavity preparation, the noise also masks the sound of the dental drill, thereby removing a source of conditioned anxiety. The music promotes relaxation, and the noise, which sounds like a waterfall, also has a relaxing effect. When both music and noise are presented, the music can be followed only through concentration; it diverts attention from the dental operation. Patients enjoy having control over the massive acoustic stimulation; in their earlier experiences in dental offices, control of the situation had seemed entirely out of their hands. The procedure provides a needed channel of communication between the patient and the dentist: the dentist can judge the patient's state of anxiety or discomfort by noting whether the patient is using music or noise, and by observing the intensity level of the signal. All the foregoing factors appear to be important, different ones predominating in different situations and for different patients. Suggestion also plays a role, the significance of which has been difficult to estimate.

The results obtained in dental operations suggest that audio analgesia may be effective also in clinical medical situations. Preliminary observations have been made with the cooperation of physicians in the Boston area. The sources of pain included left heart catheterizations, removal of toenails, labor and childbirth, and the removal of a polyp from the shoulder of one of us. The audio procedure was effective in over two-thirds of these applications. When it was not effective, the patient was not relaxed, or the pain was well developed before the sound was turned on, or it was not feasible to continue intense stimulation throughout the operation. Exposure to intense acoustic stimulation must be carefully controlled in order to avoid the possibility of producing damage to hearing.

Audio analgesia is more effective against some kinds of pain than others. In the polyp removal, there was sharply localized pain ("pinprick") at the time of the incision and again when the suturing needle passed through the skin. The pain was clearly recognizable, but quite small and inconsequential. During the remainder of the operation, there was nothing that could be called pain—only pressure and tension. Some patients report no pain at all when the noise is on at high intensity. Others say that there is detectable pain, but that "it doesn't hurt."

Efforts to examine the phenomenon in the laboratory have encountered the difficulties noted in other nonclinical studies of analgesia (*4*). If the subject pays attention to the nociceptive stimulus and reports upon the magnitude of the resulting subjective pain, the effect of acoustic stimulation is usually small. It is possible, however, by duplicating the clinical context as nearly as possible, to set up demonstrations in which the subjective magnitude of a pain (deep pain of slow onset) is clearly modulated by the turning on and off of intense sound.

The pain-reducing effect of intense stimulation is not restricted to the auditory modality. Effect of vibratory stimulation has been observed by Weitz (*5*) and Wall (*6*). In our laboratory, Baruch and Fox recently demonstrated that a bright flash of light can inhibit the pain response to a localized electrodermal shock.

In thinking toward an explanation, we note that parts of the auditory and pain systems come together in several regions of the reticular formation and lower thalamus. The interactions between the two systems are largely inhibitory. Both the direct suppressive effect and the effects mediated through relaxation, reduction of anxiety, and diversion of attention, can be explained by assuming that acoustic stimulation decreases the "gain" of pain relays upon which branches of the auditory system impinge. The behavior of an analogue-computer simulation of the hy-

Instructions for preparing reports. Begin the report with an abstract of from 45 to 55 words. The abstract should *not* repeat phrases employed in the title. It should work with the title to give the reader a summary of the results presented in the report proper.

Type manuscripts double-spaced and submit one ribbon copy and one carbon copy.

Limit the report proper to the equivalent of 1200 words. This space includes that occupied by illustrative material as well as by the references and notes.

Limit illustrative material to *one* 2-column figure (that is, a figure whose width equals two columns of text) or to *one* 2-column table or to *two* 1-column illustrations, which may consist of two figures or two tables or one of each.

For further details see "Suggestions to Contributors" [*Science* 125, 16 (1957)].

pothesized process reflects the characteristics of audio analgesia observed in clinic and laboratory. Moreover, in a recent letter, Mountcastle reports that he has found, in the posterior group nuclei of the thalamus and in the cerebral cortex, pain-evoked neural activity that is suppressed by acoustic stimulation (*7*).

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18 March 1960

Arizona's Oldest Cornfield

Abstract. Flood-plain alluvium at the Cienega site, San Carlos Indian Reservation, central Arizona, contains two preceramic and one ceramic cultural horizon. Pollen of *Zea mays* appears in each, substantiating previous early records of agriculture in the American Southwest. Prehistoric cultivation extended through at least 2000 years, ending in the late 15th century.

The earliest southwestern record of corn (*Zea*), dated at about 5600 years ago, comes from Bat Cave, New Mexico (*1*). Confirmation of early agriculture associated with the preceramic Cochise culture appeared in Tularosa and Cordova caves, New Mexico (*2*). Pollen analysis of flood-plain sediment enables us to extend the record of Cochise cultivation into an adjacent part of Arizona and to locate a probable prehistoric cornfield.

While excavating the Cienega Creek site in Arizona, Haury collected sediment samples which we have used to construct a pollen profile (*3*). *Zea* appeared in 15 of 19 levels, a total of 42 pollen grains (Table 1). The locality lies along the Cienega Creek about 4 miles southeast of the University of Arizona Archaeological Field School on the San Carlos Indian Reservation, Graham County. Here at about 6200 feet elevation a natural meadow of grasses and forbs, locally with cattail and sedge, is surrounded by upland forest of ponderosa pine, pinyon, juniper, and oak. Stream erosion during historic time cut into alluvial deposits of the meadow, exposing prehistoric remains.

Traces of human activity (hearths, cremations, shallow wells, and artifacts) can be assigned to three periods: to Chiricahua and San Pedro stages of the Cochise culture, and to Mogollon-Pueblo occupation, roughly dated at A.D. 1000 (*4*).

Radiocarbon dates of various strata led to discordant results. University of Arizona determinations of bed D-1 (roughly equivalent to level 245 to 260 in Table 1) average about 4200 years before the present and exceed by 1700 years Michigan dates of the same material. On the basis of cultural chronology, Haury considered the former more reasonable. Regardless of age, pollen analysis provides an effective monitor of *Zea* cultivation. It supports the archeologist's suspicion that prior to the rise of a ceramic tradition corn was in general use throughout the Southwest (*5*), as it was in northern Mexico (*6*).

Admittedly, fossil Maydeae pollen is not identified quite as conclusively as are cobs or kernels. We base our determination on the absence of any similar large native grass pollen in the modern pollen rain trapped in cattle tank sediments in the study area (N = 22,000); and on size-frequency measurements of grass pollen in three fossil strata compared with the size of alleged *Zea* plotted as a histogram on the same abscissa (Fig. 1). The largest native grass encountered in our count was 48 μ long with a pore diameter of 10 μ.

Compared with other Maydeae (*7*) the great size range of our measurements (55 to 104 μ total length, 11 to 17.6 μ diameter of annulus) and low average axis-annulus ratio ($\bar{X}$ = 5.7, N = 35) suggests a mixed population. For two reasons we hesitate to make such a claim, that is, that some of this pollen is derived from *Tripsacum* or teosinte. First, these are unknown from the local archeological record; second, experimental evidence shows that pollen size in *Zea* is highly susceptible to environmental control (*8*). A third possible explanation for high variability in Point of Pine *Zea* pollen is poor preservation in alluvial sediment with attendant breakage, folding, shrinkage, and stretching.

Table 1. Corn at the Cienega Creek site. N = estimated total number of pollen grains scanned for *Zea* at each level.

Depth (cm)	*Zea*		
	Pollen grains (No.)	Frequency (%)	N
	Mogollon		
20	1	0.03	3500
45	1	0.18	560
65	1	0.13	760
80	1	0.74	136
	Cochise culture—Preceramic San Pedro		
110	0		1900
125	0		1520
145	4	0.03	14900
165	0		1820
175	1	0.03	3400
195	3	0.07	4420
210	2	0.15	1300
225	3	0.23	1320
235	1	0.07	1390
	Cochise culture—Preceramic Chiricahua		
245	7	0.47	1500
250	7	0.22	3200
260	1	0.03	3200
270	6	0.14	4400
280	3	0.04	6900
Totals	42	0.07 (av.)	56126

Although it appears that corn was cultivated along the Cienega Creek for at least 2000 years, the archeological record at Point of Pines reveals population decline and abandonment by the

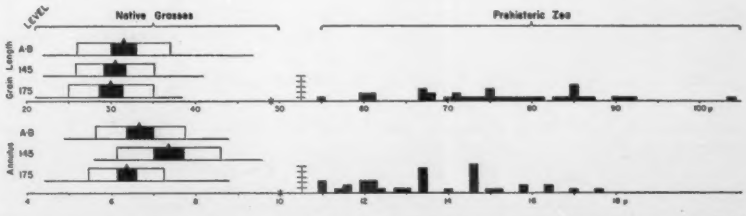

Fig. 1. Size frequency of grasses and *Zea* from the Cienega site. Mean, range, standard deviation (white bar), and twice standard error of mean on either side of the mean (black bar) represent three different populations of fossil grass pollen (N = 50 in each). The largest grass measured during analysis of the Cienega site profile is shown by an asterisk. Grains with total length equal to or exceeding 60 μ, or with annulus equal to or exceeding 11 μ in diameter, are considered Maydeae (*Zea*). Their measurements are plotted as a histogram on the same abscissa as the native grass populations.

mid-15th century (*9*). The region has not been occupied by agricultural Indians in historic time. During the last 500 years a climatic shift, possibly involving summer monsoon rainfall, may have left the area unsuitable for corn cultivation (*10*).

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2 March 1960

Environmental Factors Influencing Progeny Yields in Drosophila

Abstract. Progeny counts in *Drosophila melanogaster* were found to be correlated with barometric pressures. Addition of a sublethal chemical to the culture produced a higher correlation coefficient, whereas growth of the culture in an electric field reduced the correlation well below the level of significance. The electric field appeared to have a protective action.

Three variations in environmental conditions were found to influence progeny yields in cultures of *Drosophila melanogaster*. The factors studied were growth in an electric field, chemicals introduced into the media, and diurnal variations in barometric pressure. We believe that the effect of the electric field is a new finding. Atmospheric pressure effects on progeny yields also appear not to have been previously reported. A. F. Brown (*1*), however, has shown a relationship between barometric pressure and a cyclic pattern in the metabolic rates of various organisms.

In the initial phases of the studies, the flies were examined for external mutant effects of phenotypic variations. After several generations had been produced without observed changes, the data were re-examined in terms of variations in progeny yields. All of the cultures in a given generation series were prepared and examined in the same manner, thus allowing a direct comparative analysis. Simple crosses were made (at 22°C) with wild type and a white eye mutant. A banana culture medium was used (*2*) with 0.1-percent mold inhibitor (*3*). Three adult pairs were left in the culture 7 to 9 days, and counting was continued until 22 days after the initial mating date.

Immediately preceding an anticipated low barometric pressure in October, 1959, wild type cultures were started almost daily, extending through the "low" period and into November, 1959, or until higher pressures occurred. The total number of flies that emerged during the first 6 days of hatching are plotted as the broken line in Fig. 1. The barometric pressure on the mating date is shown as the solid line. With increasing barometric pressure there is an increase in the progeny yield. These curves have similar contours and although the peaks and valleys do not exactly coincide, there are similarities.

The relationships were also critically analyzed with a statistical correlation coefficient (r) for ungrouped data, given by Freund (*4*) as

$$r = \frac{n\Sigma F_i P - (\Sigma F_i)(\Sigma P)}{[n\Sigma F_i^2 - (\Sigma F_i)^2]^{\frac{1}{2}}\ [n\Sigma P^2 - (\Sigma P)^2]^{\frac{1}{2}}} \quad (1)$$

where F_i represents the total number of flies per filial generation, n represents the total number of generations, and P represents the barometric pressure. These data are considered to be significant at the 95-percent confidence level if

$$r > \pm 1.96 / (n-1)^{\frac{1}{2}}.$$

The r value for the seventeen F_1 generations shown in Fig. 1 was 0.51. These data are significant at the 95-percent confidence level.

Repeated filial generation crosses with the same wild type cultures also disclosed similarities between the contours of the progeny curves and barometric pressure. A total of 25 control cultures extending over an 8-month period gave an r value of 0.48, significant at the 95-percent confidence level. A greater degree of correlation was obtained by taking an average of three daily barometric pressure readings over the 72-hour period covering the day before, of, and after the initial mating of each generation.

A more pronounced correlation was

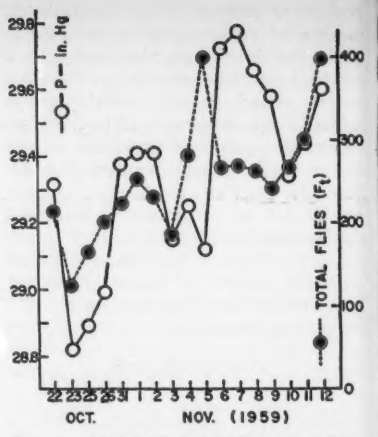

Fig. 1. Variation in wild type progeny with barometric pressure (P represents pressure on date of mating).

obtained by adding a sublethal chemical to the culture media. The material used was protocatechuic acid (3,4-dihydroxybenzoic acid), and the amount tolerated was about 0.5 percent by weight. Marked variations were observed in the progeny yields. In some cases a sequence of generation matings was suddenly terminated because of very low counts: one or two flies and occasionally none.

The progeny yields of 11 successive generations of two different cultures exposed to the protocatechuic acid are shown in Fig. 2. The barometric pressure values are averages of the 72-hour

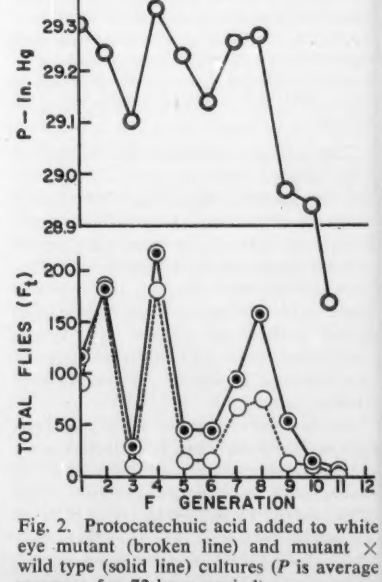

Fig. 2. Protocatechuic acid added to white eye mutant (broken line) and mutant × wild type (solid line) cultures (P is average pressure for 72-hour period).

period mentioned above. The initial mating in one case was with the white eye mutant and in the other case a white eye crossed with wild. It was interesting to note that these mutant crosses survived and successfully reproduced, with the chemical added, whereas wild fly cultures generally died out in one or two generations. This was true regardless of changes in the barometric pressure. In fact it was very difficult to obtain an F_1 from wild flies with the added protocatechuic acid. The coefficient of correlation for the white eye mutant is 0.67, and for the mutant × wild type cross, 0.76; both curves are significant at the 95-percent confidence level.

Placing the cultures in an electric field appeared to have a different effect on the progeny yield. The culture bottles were 11.5 cm high, and 4.8 cm in diameter. Semicircular electrodes were formed from copper sheeting (0.05 cm thick) and mounted on 0.5 cm thick plexiglass bases. The electrodes were 8 inches high and contacted diametrically opposite sides of the bottles. The separation at the sides of the copper plates was 2.5 cm. A 12×10^3 volt potential was applied across the electrodes with a power supply with full-wave rectification. Because of the shape of the electrodes and the inserted bottle, it was difficult to determine the exact field strength inside the bottles; however, by making some simplifying assumptions, the electrostatic field strength was estimated to be 7×10^2 coul.

The progeny yields of cultures in the electric field were, on the average, higher, and the effect of barometric pressure was considerably reduced. A coefficient of correlation of 0.08 was obtained between barometric pressure and 16 consecutive wild type generations, in the electric field. The barometric pressure values were again averaged over the 72-hour interval. The electric field appears to provide a certain amount of protection and reduces the variations found outside the field.

There is one possible explanation for this electric field effect; however, this is mostly conjecture on our part. It was felt that perhaps the barometric pressure variation is really reflecting another unknown factor. This factor might be connected with variations in the terrestrial electric fields at high altitudes, which are known to affect our general weather patterns (*5*), and which might also influence the progeny yields. Flies in the electric field are, in a sense, protected or shielded from external fluctuations. It is suggested that it might be interesting and profitable to study the reactions of more complex organisms with induced carcinogenic growths, in the electric field. Humphrey and Seal (*6*) have recently shown the effects of an external electric field on the growth of tumors in mice. We are continuing our studies on the comparison of flies grown in various environments.

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7 March 1960

Observations on the Behavior of the Porpoise Delphinus delphis

Abstract. Common porpoises have been observed in January, in the area of the Hudson Canyon, feeding on fish that escaped an otter trawl. An echo-sounder also recorded, in one instance, a descent of a porpoise to a depth of 200 feet in less than 2 minutes.

On the afternoon of 2 February 1959, we had an opportunity to observe two porpoises (*Delphinus delphis* Linnaeus) feeding on fish escaping from a trawl net, and to record them at depth on an Edo echo-sounder (Navy designation AN/UQN-1B; apex of sound beam about 30° between the half-power points). The observations were made aboard the R/V *Albatross III* (cruise 126, station 10-6, lat. 39°48′ N, long. 72°28′ W, depth of water 200 feet, surface temperature 45.9°F) in the area of the Hudson Canyon. At this season in these waters the observation of this species is, in itself, worthy of note.

Immediately following the haul back of the otter trawl, two porpoises appeared and began to feed on specimens of red hake (*Urophycis chuss* Walbaum) and scup (*Stenotomus versicolor* Mitchill) that had escaped from the net. These fish were disabled and were floating belly up as a result of their air bladders being decompressed. The porpoises were circling about at the surface and usually passed by each fish at least once before turning and picking it up.

The porpoises did not appear to be the least bit alarmed by the boat or the many people moving around on deck. They passed within a few feet of the boat several times.

Both porpoises disappeared for short intervals, at which times distinctive echo sounding records were obtained. These tracings are unique in our experience; we feel, without doubt, that they represent echoes from the porpoises (see Fig. 1). Both porpoises once descended together to the bottom, a depth of 200 feet, in about 2 minutes, and returned to the surface in less time than that. One of them dived to about 150 feet shortly thereafter and stayed at that general depth for a short period before returning to the surface.

It is not uncommon for either marine biologists or commercial fishermen to observe sharks feeding on fish that have

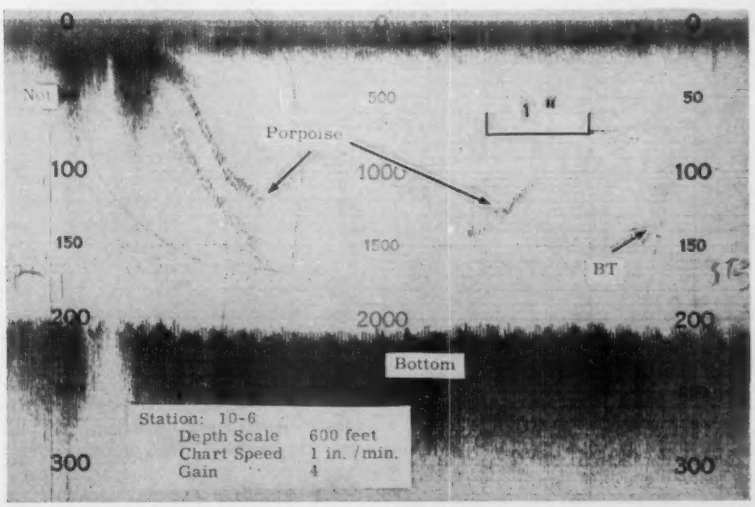

Fig. 1. Portion of echo-sounder chart showing echoes from common porpoises and from bathythermograph (BT). The vessel was hove to at the time this record was made.

escaped from trawl nets near the surface. To our knowledge, however, this is the first record of porpoises behaving similarly. Records of the depths to which porpoises descend are rare. Since porpoises presumably retain some air in their lungs when submerged, echoes of the same sort observed from fishes with sizable air bladders may be expected, in addition to echoes from the large surface area presented by a porpoise. Further observations with echo-sounders may add considerably to our knowledge of the depths to which these interesting mammals descend.

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14 March 1960

Osmotic Pressure and Aqueous Humor Formation in Dogfish

Abstract. Comparative chemical analyses show the aqueous humor of smooth dogfish has lower concentrations of urea and trimethylamine oxide than the blood plasma. Freezing-point depression determinations demonstrate that the aqueous humor has a correspondingly lower osmotic pressure. It would appear that water entering the aqueous humor from the plasma is moving against an osmotic gradient.

Current theories on aqueous humor formation in higher vertebrates generally contend that some ion (for example, sodium) is pumped across the ciliary epithelium from the plasma, and that water then follows by osmosis (*1*). Carbonic anhydrase appears to play an important role in this process, and its action has been compared to other water balance systems, such as that in the kidney (*2*). Aqueous humor usually has lower concentrations than plasma for those constituents which cross the ciliary epithelium more slowly than water—namely, urea, glucose, amino acids, and protein. Such substances do not attain a diffusion equilibrium since the rapid drainage of the fluid is nonspecific and allows all the constituents to be removed at the same bulk rate. Consequently, a steady-state distribution ratio (aqueous concentration: plasma concentration) is reached which is less than unity and is characteristic of the particular substance. Conversely, constituents which are pumped or secreted into the aqueous humor have distribution ratios greater than 1 (*3*).

As is well known, elasmobranch fishes retain huge quantities of urea and trimethylamine oxide in their plasma, thereby elevating the osmotic pressure of the plasma above that of sea water (*4*). The study reported here was undertaken to determine the distribution ratios of these substances, since a ratio of less than 1 would result in a potentially large osmotic pressure difference between the blood plasma and the aqueous humor. For this reason, it was thought that the operative mechanisms in the elaboration of aqueous humor might be exaggerated in these fishes to compensate for the lesser osmotic contribution of urea and trimethylamine oxide in their aqueous humor.

Table 1 gives the results of chemical and physical analyses on the aqueous humor and plasma of 90 smooth dogfish (*5*). Urea and trimethylamine oxide (TMAO) reach steady-state distribution ratios of 0.93 and 0.88, respectively, the combined deficit (plasma concentration-aqueous concentration) being approximately 34 mmole/kg of water. Theoretically this could represent a difference in osmotic pressure of about 580 mm-Hg. The only component of dogfish aqueous humor in great enough excess to be osmotically important is bicarbonate ion, whose distribution ratio averaged 2.5, representing a surplus of about 9 mmole/kg of water. The higher concentration of bicarbonate ion in the aqueous humor is nullified, however, by a lower chloride ion concentration, and it would appear that an exchange of these two anions occurs, as it apparently does in other carbonic anhydrase mediated systems. The *p*H is consistently high. Sodium ion attains a distribution ratio concordant with an estimated Donnan equilibrium. Analyses on other constituents failed to demonstrate any substance or substances in great enough surplus to balance the urea-trimethylamine oxide deficit, even though more than 98 percent of the respective dry weights were accounted for.

Freezing-point depression measurements made with a Fiske osmometer (sample size, 0.2 ml) showed that the osmotic pressure of the aqueous humor was about 25 milliosmoles lower than that of plasma, suggesting that the passage of water from the plasma into the eye of this fish is against an osmotic gradient. The smallest difference found in 14 fish tested was 14 milliosmoles. The measured osmotic pressures compared favorably with those calculated from the totaled results of chemical analyses when the effect of ionic strength on activities was considered.

It seemed important to find out how similar the aqueous formation in this species is to the process in higher vertebrates. It is significant that elasmobranchs are the only fishes which possess a ciliary body (*6*). The ciliary body contains carbonic anhydrase, as it also does in mammals, and the specific inhibitor of this enzyme, acetazoleamide (Diamox), can lower the distribution ratio of bicarbonate ion to less than 1 when it is injected intravenously in the dogfish (*7*). We regard the fact that most of the distribution ratios of substances measured were characteristic of higher vertebrates (namely, an ascorbic acid excess, very low protein, and so on) as circumstantial evidence that the aqueous humor formation in these fishes follows a pattern similar to that in mammals. In addition, penetration studies (plasma to aqueous) with Na^{24} showed that the turnover rate for this isotope is about 1 percent per minute, a value similar to values found for the rabbit (*3*).

Table 1. Comparison of properties of aqueous humor and blood plasma in *Mustelus canis*.

Constituent	Fish (No.)	Concentrations: Av. (mg/100 ml)		Concentrations: Av. mmole/kg of H_2O*	
		Aqueous	Plasma	Aqueous	Plasma
Urea	11	1835	1898	320	342
TMAO	12	754	832	85	97
Sodium	12	621	613	279	288
Chloride	9	867	886	256	270
Bicarbonate	23			15	6
Potassium	4	27	31	7	8
Magnesium	4	7	7	3	3
Calcium	3	12	18	3	5
Phosphate	3	12	19	1	2
Sulfate	2	37	27	4	3
Glucose	8	120	210	7	13
Protein	6	22	2850		
Amino acids	8	15	56		
Ascorbic acid	9	2.4	.8		
Total		4331	7438	980	1037
Osmotic pressure (mm-Hg)	14			935	962
Average dry weight (mg)	4	4470	7510		
*p*H	8			7.86	7.33

* To the nearest millimole. Not corrected for Donnan effect.

The possibility that some sea water may be entering the eye across the cornea cannot be categorically rejected, since the aqueous humor is slightly hyperosmotic with respect to the sea water in which these fish were maintained. The composition of the fluid does not resemble sea water, however, and this would seem to indicate that any inward movement across the cornea would have to be essentially a movement of pure water and not of ions. For example, the concentration of magnesium ion is essentially the same in plasma and aqueous (3 mmole/kg), a value 20 times smaller than that for sea water (61 mmole/kg). A second possible explanation of these data could be based on the hypothesis that some constituent is reabsorbed from the aqueous humor, perhaps across the iris.

In the absence of evidence supporting either of the above two interpretations, the following tentative speculation is introduced: (i) in the dogfish, water is apparently moving from the plasma into the aqueous humor against an osmotic gradient; (ii) the additional work needed for this process is reflected in the unusually high bicarbonate ion concentration in dogfish aqueous humor; and (iii) carbonic anhydrase is instrumental in this process (*9*).

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5. The dogfish used in the analyses were *Mustelus canis*. Aqueous samples were taken from the pupillary region; blood samples, from the caudal vein.
6. G. Walls, "The Vertebrate Eye and Its Adaptive Radiations," *Bull Cranbrook Inst. Sci. No. 19* (1942).
7. T. H. Maren reported this effect in the spiny dogfish *(Squalus acanthias)* [*Federation Proc.* **17**, 391 (1958)]. We are indebted to him for his help in suggesting dosages and sampling times which enabled us to obtain similar results in our own fish. It should be noted, however, that Maren reports a somewhat lower distribution ratio (in untreated fish) for bicarbonate in the spiny dogfish.
8. The sea water in our tank had an osmolarity of 910 ± 5 milliosmoles.
9. This project was supported by the Office of Naval Research and by the Boston Lions Club Eye Research Fund. We wish to thank Alfred Marshak for suggesting certain aspects of this work, and Jin Kinoshita and Oliver Cope for the use of certain critical pieces of apparatus.

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Control of Vocal Responding in Chickens

Abstract. Schedules of reinforcement were shown to control the rate of chirping by chickens in the same way as other motor responses in subhumans. Under a discrimination procedure, chickens responded selectively to the visual stimulus associated with food reinforcement for chirping. Control experiments demonstrated that food influenced the rate of responding because it was presented dependent on the chick's vocalizing and not because it had an innate eliciting or "emotionalizing" power.

The appropriate conditioning procedures for modifying subhuman vocalizing have, until recently, been little studied and poorly understood (*1*). It has not been clear whether respondent or operant conditioning techniques (*2*), or both, are suitable for the control of this behavior (*3*). The studies reported here (*4*) show that the rate of chirping by a hungry chicken can be controlled by schedules of presentation of food. The temporal patterns of vocal responding thus obtained are similar to those found with other operants under schedules of reinforcement, for example, key pecking in the pigeon and bar pressing in the rat, and a wide variety of human motor responses.

The subjects for these experiments were two Bantam chickens which were run individually at 80 percent of their ad libitum body weight (*5*) in sessions that lasted from 30 minutes to 6 hours. The experimental space consisted of an 8-in., cubical, sound-insulated compartment. An opening in one wall provided access to a food tray whose presentation was scheduled automatically. A microphone mounted in the ceiling of the compartment controlled a voice-operated relay which reacted to about 95 percent of the audible chirp responses by sending pulses to programming and recording equipment.

The rate of vocal responding was observed under two experimental and five control conditions. Figure 1 summarizes the data obtained from one bird; a second bird gave similar results. The first two studies were controls in which the rate of chirping was observed under conditions of no presentation of food ("operant level") and continuous presentation of food ("free-feeding") (*6*). The stabilized rate of responding under each of these two control conditions is shown in Fig. 1. Because chirping decelerated in the early stages of the operant-level session, it may be inferred that an average rate lower than 24 responses per minute would have been obtained for this condition if the session had been extended beyond its half-hour duration. The average rate during a 1-hour free-feeding session was 27 responses per minute. The chickens chirped both while pecking at the grain in the tray and while not eating.

In a third experiment the presentation of food was contingent on responding; a fixed-ratio 20 schedule of reinforcement was employed (*7*). Under this schedule, the food tray, containing meal, was presented for 4 seconds after every 20 chirps. An extremely high rate of responding was generated by this procedure; the average rate observed in a 1-hour sample after 10 hours of conditioning was 115 responses per minute. A typical sample of the performance obtained with the fixed-ratio 20 schedule is shown in Fig. 2, in which cumulative chirp responses are plotted as a function of time. The diagonal marks on the curve indicate the presentation of food. The interreinforcement time under this schedule averaged about 16 seconds.

In the fourth experiment, a control, food was presented with the same frequency and duration as obtained under

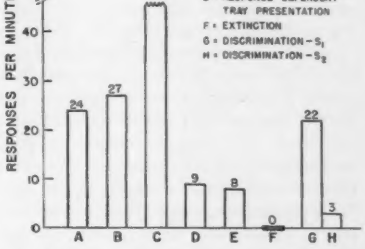

Fig. 1. Rates of chirping obtained under control and experimental conditions with one Bantam chicken.

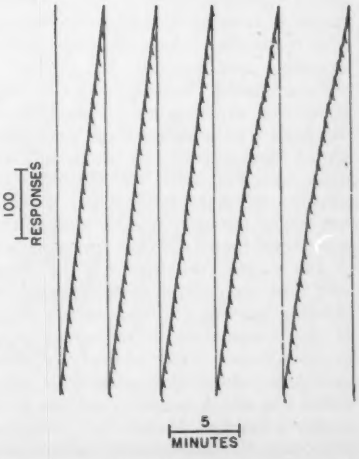

Fig. 2. Cumulative response curve for the chirp response of one chicken under a fixed-ratio 20 schedule of reinforcement.

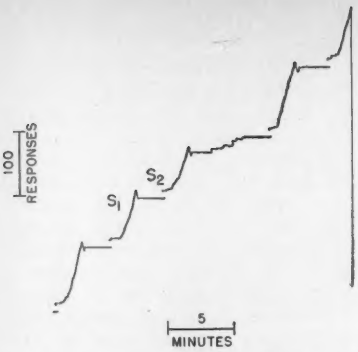

Fig. 3. Cumulative response curve for the chirp response of one chicken under a discrimination procedure.

the fixed-ratio schedule: 4 seconds of access to the food tray every 16 seconds. However, this periodic feeding was provided independent of responding. Figure 1 shows that the rate under periodic feeding (bar *D*) was less than 10 percent of the rate observed under fixed-ratio (20) (bar *C*). Apparently, it is the response requirement rather than the food presentation alone that is responsible for the high rate of responding under fixed-ratio schedule.

A fifth experiment demonstrated that the high rate of vocalizing obtained under fixed-ratio reinforcement was not simply the result of the stimulus change provided by response-dependent tray presentation. The procedure for this control condition was the same as that for the fixed-ratio condition, a 4-second tray presentation after every 20 responses, except that the food tray was now empty. The average rate of responding in the fifth hour under this condition is shown in Fig. 1, bar *E*. When a formerly neutral stimulus is paired repeatedly with a primary reinforcer it acquires a temporary control over responding which dissipates if the stimulus continues to be presented without further pairing (*8*). In the vocalizing experiments reported here, the food tray and the food were presented concurrently on thousands of occasions. The effect on the rate of chirping of discontinuing this pairing was slight initially. By the second experimental hour, however, presentation of the empty tray had, for the most part, lost its control over the rate of chirping (see Fig. 1). The terminal effect of the presentation of the empty tray may be compared to the effect of simple extinction—the sixth experimental condition—in which neither food nor tray is ever presented. Under the extinction procedure, the response rate fell to zero within a half hour.

In the final study, food was made contingent on responding during one stimulus and on not responding during a second stimulus. When a red light was on in the subject's compartment, food presentation was contingent on the first chirp after 2 minutes had elapsed. Following one reinforcement on this fixed-interval schedule (*7*), a green light was turned on and food presentation was then contingent on a pause in responding of at least 2 minutes. These conditions alternated. Each of the birds learned the stimulus discrimination within an hour. High rates of responding were observed during the stimulus that was paired with fixed-interval reinforcement (S_1) and low rates of responding were observed during the stimulus that was paired with reinforcement for not responding (S_2). When cumulative chirps in S_1 are plotted as a function of time, a pattern of responding is obtained that is typical of operant performance under fixed-interval schedules of reinforcement: the interval begins with a pause and the rate increases as the time of reinforcement approaches. Figure 3 shows a sample of the cumulative response record obtained during a stable discrimination performance (average rates for this session appear in Fig. 1, bars *G* and *H*). The average rate of responding during the fourth hour of training on the discrimination procedure was 22 responses per minute in S_1, and three responses per minute in S_2; the modal rate of responding in S_2 was zero.

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1. Some anecdotal information has been available, however. See, for example, O. H. Mowrer, *Learning Theory and Personality Dynamics* (Ronald Press, New York, 1950), pp. 688–726.
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3. Skinner has suggested that "vocal behavior below the human level is especially refractory" to operant conditioning: B. F. Skinner, *Verbal Behavior* (Appleton-Century-Crofts, New York, 1957).
4. This research was supported in part by a grant from the National Science Foundation, and was carried out during the tenure of a predoctoral fellowship from the National Institute of Mental Health, U.S. Public Health Service.
5. The chicks were 5 weeks old at the start of this research. After 5 months there was a marked change in topography and decline in rate of chirping in the home cage and the experiments were discontinued. Because the chicks were growing during this 5-month interval, continued maintenance of 80 percent of their ad libitum body weight, as initially determined, would have resulted in starvation. Therefore, every month the birds were given free access to food in their home cage for 2 days and a revised "running weight" was computed.
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9 February 1960

Descending Projections to Spinal Motor and Sensory Cell Groups in the Monkey: Cortex versus Subcortex

Abstract. In rhesus monkeys, corticospinal fibers have been found to terminate in motor, sensory, and internuncial cell groups. The present study reveals that the subcorticospinal fibers terminate primarily on internuncial neurons. Possible mechanisms involved in transmission of cortical and subcortical influences on motor and sensory cell groups are discussed.

Previous neuroanatomical studies in the rhesus monkey (*1*) revealed that pre- and postcentral corticobulbar and corticospinal projections terminate in motor, internuncial, and sensory cell groups (Fig. 1*A*). The motor and internuncial cell groups receive precentral fibers primarily. The sensory cell groups of the spinal trigeminal complex and the spinal posterior horn receive postcentral fibers primarily, while the nuclei cuneatus and gracilis receive both pre- and postcentral fibers. The precentral fibers to motor and internuncial neurons subserve motor functions and in all likelihood represent "direct" and "indirect" corticomotoneuronal pathways (see *2*). The pre- and postcentral projections to the sensory nuclei, on the other hand, probably represent cortical sensory feedbacks. This hypothesis is supported by physiological demonstrations of a cortical influence on these sensory nuclei (*3*). Since the brain stem likewise influences the sensory nuclei (*3*, *4*), the cortical influence on these nuclei was first thought to be mediated through the reticular formation (*3*). However, in cats, a direct cortical pyramidal influence upon the nuclei cuneatus and gracilis (*5*) has been demonstrated physiologically, in striking agreement with the anatomical observations (*6*).

In view of the physiological findings of subcortical influences on sensory nuclei, subcorticospinal pathways were studied anatomically.

In nine rhesus monkeys, lesions were made in the medulla oblongata (level of hypoglossal nucleus). The sum of these lesions interrupted all the longitudinal bundles throughout the medullary *tegmentum*, leaving the pyramidal tract and its immediate vicinity untouched. The descending fiber degeneration was studied, by use of the Nauta-Gygax technique (*7*). As planned, this study revealed the terminal distribution of the descending fibers. Therefore, contrary to the classical approach, the descending fibers will be grouped strictly according to their termination. The origin of these fibers will be studied later.

Descending fibers were encountered throughout the tegmentum below the lesions. Among these fibers two types

could be distinguished: (i) long fibers descending throughout the entire spinal cord; (ii) short fibers reaching the cervical levels.

The long bundles were encountered in the medial, ventral, and ventrolateral parts of the medullary cross sections. In the lateral parts of the tegmentum, medial to the spinal trigeminal complex, only short fibers were found.

From their terminal distribution (characteristically seen at a great distance from the lesion, for example, the lumbar region), two main groups of long descending systems could be distinguished. One was a lateral, long, subcorticospinal system, passing through the ventrolateral parts of the medullary cross section and descending through the lateral spinal funiculus (Fig. 1*B*, dotted area). This system, which contains undoubtedly many rubrospinal fibers, terminates primarily in the basal parts of the posterior horn and in the zona intermedia. The second main group was a ventromedial, long, subcorticospinal system, which passes through the ventral and medial parts of the medullary cross section and descends through the medial and ventrolateral spinal funiculi (Fig. 1*B*, area with crosses). This ventromedial system, which probably contains reticulospinal, vestibulospinal, and interstitiospinal fibers, terminates primarily in the medial and dorsomedial parts of the anterior horn and the adjacent parts of the zona intermedia. Extremely few of these long descending fibers terminate in the motoneuronal cell groups of the anterior horn or the nucleus proprius of the posterior horn. However, the most medial motor cell groups of the anterior horn might receive some long descending fibers.

The cells in the basal parts of the posterior horn and the zona intermedia, on which the lateral fibers terminate, are commonly regarded as propriospinal elements. The larger neurons in the medial and dorsomedial parts of the anterior horn, reached by the ventromedial fibers, are most likely of the same nature (*8*). In other words, both groups of long subcorticospinal fibers are distributed first and foremost to propriospinal neurons.

This distribution of the subcortical fibers to propriospinal neurons contrasts sharply with that of the cortical fibers which are distributed to sensory, motor, and propriospinal cell groups. As a consequence, while the cortical influence on the motor neurons and sensory cells in the posterior horn is direct, the influence of the subcortical centers on these cell groups (*9*, *4*) must be exerted primarily through propriospinal internuncial neurons (Fig. 1*D*).

The internuncial nature of the nerve cells in the basal parts of the posterior horn and zona intermedia with respect to motor neurons is commonly accepted. A similar internuncial relation between those cells in the intermediary region and secondary sensory cells in the posterior horn previously has been assumed (*10*). The following anatomical findings likewise suggest such a relation. Lesions of the lateral tegmentum of the medulla oblongata revealed the existence of short fiber bundles, which descend into the lateral propriospinal bundles of the cervical segments. A great many of these fibers terminate in the lateral tegmentum and its spinal homolog, the zona intermedia. In addition, some of these fibers pass to local motor neurons (hypoglossal nucleus, upper cervical anterior horn) as well as to secondary sensory cells (spinal trigeminal complex), in agreement with previous observations in Golgi material

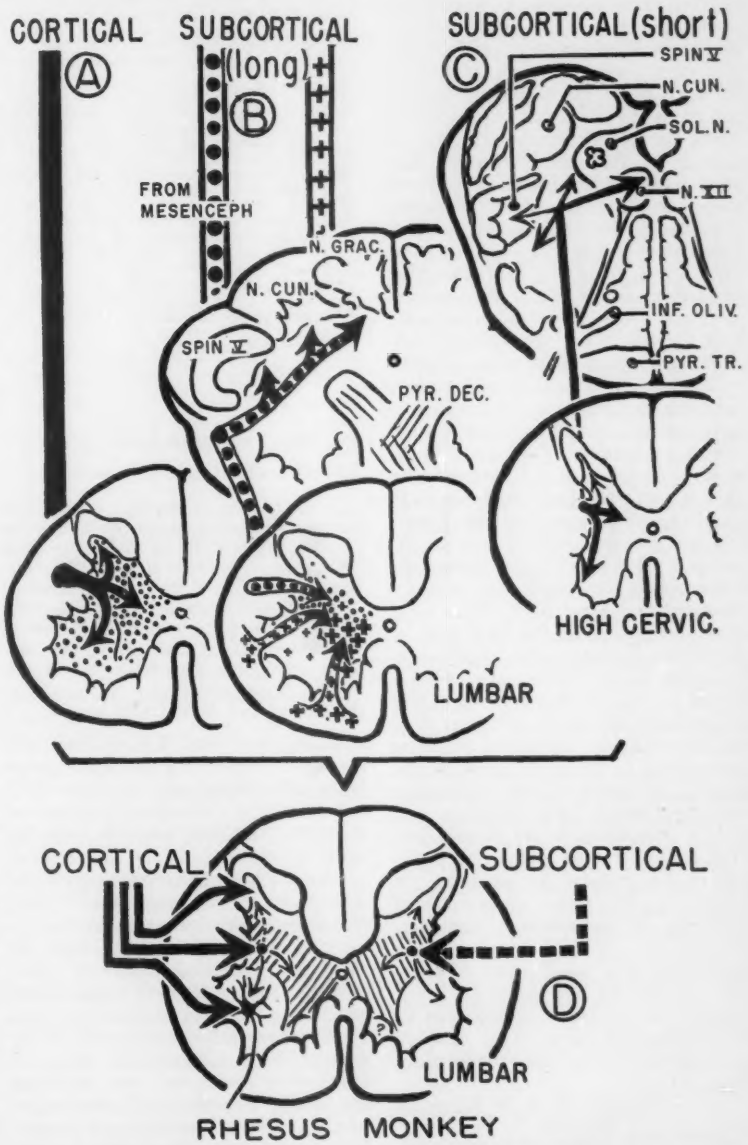

Fig. 1. Diagrams *A* and *B* illustrate spinal distributions of cortical (*A*) and long subcortical fibers (*B*). Numerous cortical fibers are distributed to the anterior and posterior horns, while only a few subcortical fibers are distributed to these cell groups. Diagram *C* illustrates distribution of some short medullary fibers descending through propriospinal bundles into the upper cervical spinal cord. Note distribution to motor and sensory cell groups. Diagram *D* emphasizes differences in termination of cortical and subcortical fibers in the spinal cord, with special reference to the influence on sensory and motor neurons. Crosshatch indicates area of interneurons.

(*11*) (Fig. 1*C*). These short fiber bundles apparently constitute the most rostral components of the lateral propriospinal system, adjoining the spinal gray matter on the lateral side. As such, the distribution of these fibers may exemplify the distribution pattern of the entire lateral propriospinal chain, originating in the lateral tegmentum and the zona intermedia and distributing among others to motor and sensory cell groups.

Were this supposition correct, we might predict that the long lateral subcorticospinal system terminating in this very zona intermedia would influence the activity of the secondary sensory cell groups in the posterior horn, in addition to its commonly accepted influence on the motor neurons. This prediction is in striking agreement with the actual findings of a systematic physiological investigation of the medullary cross section (*12*). In that study (*12*) it was found that the points from which the sensory cells in the posterior horn could be influenced dromically were located in the ventrolateral parts of the cross section, an area occupied by the lateral subcorticospinal system. Furthermore, this long, lateral, subcorticospinal system apparently also transmits a subcortical influence on the sensory nuclei cuneatus and gracilis. This is suggested by findings in additional experiments, in the rhesus monkey and the cat, with lesions in the mesencephalon. In these cases, the lateral subcorticospinal system was degenerated and some of its fibers were found to distribute to the basal parts of the nuclei cuneatus and gracilis. On the other hand, some physiological studies (*3*) suggest that the medial bulbar reticular formation influences "all" the sensory nuclei: the spinal posterior horn as well as the nuclei cuneatus and gracilis. In regard to this conclusion some reservations seem necessary (see *5*, *12*). Moreover, in the present material a distinct fiber system from this part of the reticular formation to the region of "all" these sensory nuclei could not be demonstrated. However, this does not allow us to rule out the possibilities of a transmission of reticular activity to the posterior horn through the mediation of the cells in the dorsomedial parts of the anterior horn, which receive long reticulospinal fibers (*13*).

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13. These investigations were supported by grant No. B-1131, National Institute of Neurological Diseases and Blindness, U.S. Public Health Service, and grant No. G-6434, National Science Foundation.
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8 February 1960

Possible Two-Stage Mechanism in Experimental Leukemogenesis

Abstract. The augmenting influence of urethan on leukemogenesis by x-radiation in mice has been found to operate when the urethan treatment follows the radiation, but not when the sequence is reversed. The result is in keeping with the idea that urethan acts as a promoting factor in leukemogenesis, as defined by the two-stage mechanism hypothesis of carcinogenesis. It may also have a practical bearing on leukemia development in man.

Kawamoto, Ida, Kirschbaum, and Taylor (*1*) reported a striking augmentation of leukemogenesis in mice, whether induced by x-radiation, estrogen, or methylcholanthrene, when the animals were at the same time subjected to urethan (ethyl carbamate) administration. Since urethan alone was entirely free from leukemogenic action, its augmenting influence was described by them as "co-leukemogenic."

In line with earlier investigations on co-carcinogenesis in mouse skin (see *2*), we attempted to analyze the abovementioned co-leukemogenic action of urethan in terms of the two-stage mechanism hypothesis of carcinogenesis. If the effect were due to a summation of two similar types of action (of which one was too weak to be demonstrated by itself), one would expect the result to be the same whether the urethan preceded or followed the radiation. If, however, the urethan acts as an initiator only, or as a promoter only, then the augmentation should operate in one application sequence or the other, but not in both.

Five groups of C57b1/6 mice, each comprising 75 young adults of mixed sexes, were treated as follows: one group received x-radiation alone; one received urethan alone; one received the two concurrently (to confirm the augmentation reported by Kawamoto *et al.*); one received x-radiation followed by urethan; and one received urethan followed by radiation.

The radiation was provided by a Mühler 250-kv machine (physical factors: 200 kv, 15 ma, 0.5 mm Cu and 1.0 mm Al added filter, 50 cm target mouse distance; output 49.5 r/min). The urethan was injected intraperitoneally as a 10-percent solution in distilled water. The radiation treatment (total body) was administered in 5 doses of 90 r each, at intervals of 5 days, a total of 450 r. The urethan was also administered in 5 doses at intervals of 5 days, the mice receiving 0.2 ml per dose, a total of 100 mg. In the case where the two forms of treatment were given concurrently, the urethan was given immediately before each radiation. Where the two forms of treatment were given during separate periods, the interval between the completion of the one and the commencement of the other was 2 weeks.

The results, after 30 weeks, measured from the time of the first radiation, were as follows: x-radiation alone, leukemia in 17 of 75 survivors (23 percent); x-radiation together with urethan, 26 of 50 (52 percent); x-radiation followed by urethan, 35 of 70 (50 percent); urethan followed by x-radiation, 17 of 74 (23 percent). Urethan alone, after 30 weeks, yielded no leukemia among the 61 survivors. The results, expressed in the form of leukemia incidence curves, are shown in Fig. 1.

The fact that augmentation of leukemogenesis by urethan is obtained when urethan is made to follow the radiation, but not when the sequence is reversed, is in keeping with the idea that urethan acts as a promoting agent in leukemogenesis, as defined by the two-stage mechanism hypothesis of carcinogenesis. This is all the more surprising, since, in the case of skin carcinogenesis, urethan acts as a pure initiator (*3*), while for the lungs it is a complete carcinogen (*4*).

The present experiment is being continued until all the animals die. Meanwhile, further experiments are in progress, involving lower doses of radiation, in the hope that the effect might be observed under more critical conditions—that is, with no leukemia arising in the x-ray control series, or in the group in which the radiation follows the urethan treatment. The effect

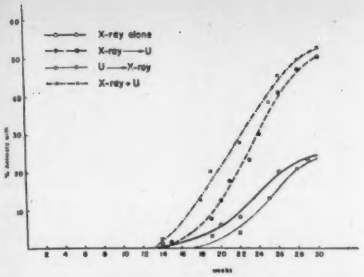

Fig. 1. Incidence of leukemia induced by x-ray alone and in various combinations with urethan.

of extending the interval between the treatments, and other variations that might throw light on the mechanism, are also being investigated.

The possibility of chemical agents acting as promoters of radiation leukemogenesis has not only theoretical interest, but also practical implications for man, with respect to the possible dangers of low doses of radiation, and the debatable question of whether there is, in fact, a threshold dose for leukemogenesis.

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14 March 1960

Influence of Anabolic Steroids on Uptake of Alpha-Aminoisobutyric Acid by Levator Ani Muscle

Abstract. Two representative anabolic steroids caused an increase in the uptake of α-aminoisobutyric acid-1-C^{14} in the levator ani muscle of rats. The distribution ratio between that muscle and the plasma was increased fourfold by the administration of a synthetic anabolic steroid and twofold by administration of testosterone propionate. The determination of this increase may serve as an indicator for the myotrophic effect of anabolic steroids.

The observation of Noall and his coworkers (*1*) that estradiol causes a threefold increase in the concentrative transfer of C^{14}-labeled α-aminoisobutyric acid (AIB) into the uterus of the rat led us to study the effect of anabolic steroids on the uptake of AIB by the levator ani muscle. Clinical studies in this laboratory (*2*) have indicated that a synthetic testosterone analog (*3*), 17-α-ethyl-4-estrene-3-β-17-β-diol-3-propionate, has strong anabolic properties; nitrogen sparing occurred within 24 hours and lasted 8 to 10 days after a single dose (2 mg/kg). It was therefore chosen as one of the test steroids, testosterone propionate being the other.

Sixty-day-old male rats of the Holtzman strain were used. Half of the group were castrated under light anesthesia. Half of the castrated and half of the noncastrated animals were then given 50 mg of the synthetic steroid per kilogram, intramuscularly, and 30 hours later all of the rats were given, subcutaneously, 1 μc of AIB, of the same specific activity (0.8 mc/mmole), per kilogram. Nine hours later the animals were killed by exsanguination. The levator ani muscles were excised, weighed, and homogenized with saline acidified to a *p*H of 5.0 with acetic acid. After centrifugation the supernatants were plated and counted in a thin-window gas-flow counter. The plasma was plated directly and counted. The results are indicated in Table 1.

Table 1. Distribution of AIB in the levator ani muscle on administration of a synthetic anabolic steroid in two different batches of rats (24 each) of the same strain. Each rat weighed approximately 250 gm.

Distribution ratio of AIB*		Mean (± S.E.)	Weight of levator ani†	
Ser. 1	Ser. 2		Ser. 1	Ser. 2
		Castrated—no steroid		
2.1	2.7	2.4 ± 0.14	48	58
2.5	2.2		45	52
		Normal—no steroid		
4.8	6.3	5.4 ± 0.63	53	60
4.8	6.7		49	60
		Normal—steroid		
7.0	7.2	7.8 ± 0.50	42	61
9.2	7.7		45	60
		Castrated—steroid		
9.9	8.2	9.4 ± 0.36	44	61
9.9	9.4		47	60

* Each value is from the pooled tissue of three rats. The ratio is counts per minute per gram of tissue: counts per minute per millimeter of plasma. † Each value is the mean for the corresponding three rats in milligrams per 100-gm rat.

The distribution ratio of the AIB in the castrated rats injected with steroid was 3.9 times as great as in the castrated animals not injected with steroid. The values for the uncastrated animals injected and not injected with steroid fell between the extremes of these two groups. In the 39 hours of the experiment there was no significant increment in the weight of the levator ani muscle. That the observed effect of the synthetic steroid on the distribution ratio of AIB was not unique to this new compound was indicated by a similar series of experiments in rats of the same strain given 50 mg of testosterone propionate per kilogram. The results for the same experimental groups were 10.2, 12.6, 16.0, and 23.0, respectively. Although the values were higher, the trend was the same, and the distribution ratio in the castrated animal treated with testosterone propionate was 2.2 times as great as in the castrated animals not treated.

The concentrative transfer of AIB into cells is apparently influenced by many endocrine substances, such as insulin (*4*), epinephrine, hydrocortisone, estradiol (*1*), and growth hormone (*1, 5*). Noall and Christensen indicate (*1*) that AIB may be considered a model for the transport of endogenous amino acids into cells and tissues and that the concentrative transfer of these amino acids, under the influence of various endocrine substances, may be the stimulus for protein formation. The anabolic androgens used in these experiments apparently also cause an increase in the transfer of AIB into the levator ani muscle, one of the target tissues of these steroids. It appears, by implication, that the myotrophic effect (and the androgenic effect) results from concentrative transfer of endogenous amino acids into the cells of the target tissues and organs.

Saunders, using the same two steroids (*6*) and the standard myotrophic test of Eisenberg and Gordan (*7*), found only a 10- to 20-percent increase in levator ani weight in 48 hours and a doubling in weight at the end of 7 days. The two- to fourfold increase in the distribution ratio of AIB within 39 hours after administration of the steroids in these experiments would appear to foreshadow the maximum myotrophic effect by at least 5 days. The AIB determination might therefore serve as a monitor for myotrophic activity and give the desired information in a much shorter period of time (*8*).

Note added in proof: Since submission of this report, two other synthetic anabolic steroids, 19-Nor-Δ-4-androstene-17β-ol-3-one-β-phenylpropionate and 17-α-methyl-17β-hydroxyandrosta-1,4-dien-3-one, have been tested in the manner described in this report. The distribution ratios of the AIB were increased 4.6 and 3.4 times, respectively, over the corresponding controls.

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12 February 1960

Effects of Ethyl Alcohol on Avoidance Behavior

Abstract. Three albino rats, trained to avoid electric shock, were stomach-loaded with from 1 to 5 ml of 32-percent aqueous solution of ethyl alcohol prior to the experimental session. Small doses produced some increases in response rates and a consistent decline in shocks received. Larger doses produced progressive uncoordination, accompanied by lower response rates and an increase in shocks received.

Avoidance behavior may be maintained by allowing each occurrence of a selected response to postpone a brief shock for several seconds (*1*). In the absence of responses, shocks are delivered at regular intervals. The steady rate of responding and the stable frequency of shocks resulting from this procedure may be altered by pharmacological agents—scopolamine, for example (*2*). In the work described in the present report (*3*) the effect of ethyl alcohol on avoidance behavior in the rat was studied.

Three male albino rats (150 days old) were used for 2 hours daily in a sound-attenuating experimental chamber equipped with a lever. Depression of the lever (a response) activated electric counters and automatic programming equipment. The floor of the chamber consisted of 1/12-in. steel bars. In the absence of lever-presses, electric shocks (3 ma) of 0.5-second duration were delivered to the rat's feet through the floor grids every 20 seconds. The polarity of each grid changed rapidly and irregularly during shock administration. Each lever-press postponed the next shock for 20 seconds. After 90 days of this procedure, each rat was deprived of food (but not water) for at least 15 hours prior to each avoidance session. On alternate days, 3 minutes before the start of the session, a dose of ethyl alcohol was introduced directly into the rat's stomach through a rubber tube inserted orally. The doses were 1, 2, 4, and 5 ml of 32-percent aqueous solution of ethyl alcohol. Each animal (each weighing about 350 gm) received these doses in a different, irregular order. The volume of alcohol per unit of weight represented by the 5 ml dose in a 350-gm rat is equaled in a 175-lb man by a dose of about ¾ quart of 100-proof liquor. Rat WM4 was not given the 5-ml dose. On control days its stomach was loaded with 2 or 5 ml of water or nothing. The number of responses and shocks in each 2-hour session were recorded.

Figure 1*a* shows for each rat the total number of lever-presses in a 2-hour session on control days (0) and after doses of 1, 2, 4, and 5 ml of 32-percent ethyl alcohol solution. The responding of each rat declines sharply between doses of 4 and 5 ml. The effect on responding after administration of lower doses is variable: for example, one rat shows a decrease and two rats an increase between doses of 1 and 2 ml.

The number of shocks in a 2-hour session is a more orderly dependent variable (Fig. 1*b*). Each animal receives fewer shocks after a dose of 2 ml of the alcohol solution than after a dose of water. The shock frequency increases above that for water after 4 ml and increases sharply after 5 ml. Low doses of alcohol increase the effectiveness of avoidance responses, although the frequency of responses may not increase (Fig. 1*a*). Higher doses of alcohol decrease the frequency of responding and increase the frequency of shocks.

The relative decrease in response rates following doses of 1 and 2 ml for rat WM5 may be brought about by the decrease in shocks received. This animal's relatively higher rate of responding under control conditions is largely accounted for by bursts of three to four responses per second during and after shocks. A decrease in shock frequency leads to a decrease in the number of such bursts.

An examination of cumulative records of responses during control sessions (water or no dose) reveals a steady rate of responding throughout both hours of the session after a gradual acceleration at the start of the session. A dose of 1 ml produces slight irregularities during the first hour, but the record during the second hour is indistinguishable from the control record. After doses of 2 and 4 ml, responding accelerates markedly and ceases for about 1 minute early in the first hour. The onset of the acceleration is earlier after a dose of 4 ml than after a dose of 2 ml. After 5 ml, responding begins at a rate higher than the control rate.

Extended periods of no responding begin near the end of the second hour after 4 ml and near the end of the first hour after 5 ml. Only one response was made by rat A3 after the end of the first hour with a 5-ml dose, and the experiment was terminated after 27 consecutive shocks had been received. In computing the number of responses emitted and the shocks received (Fig. 1) for rat A3, it was assumed that no further responses would have been emitted and that the shock frequency would have been maximum for the remainder of the second hour of the session. This animal died 12 hours later without recovering from the effects of 5 ml of alcohol.

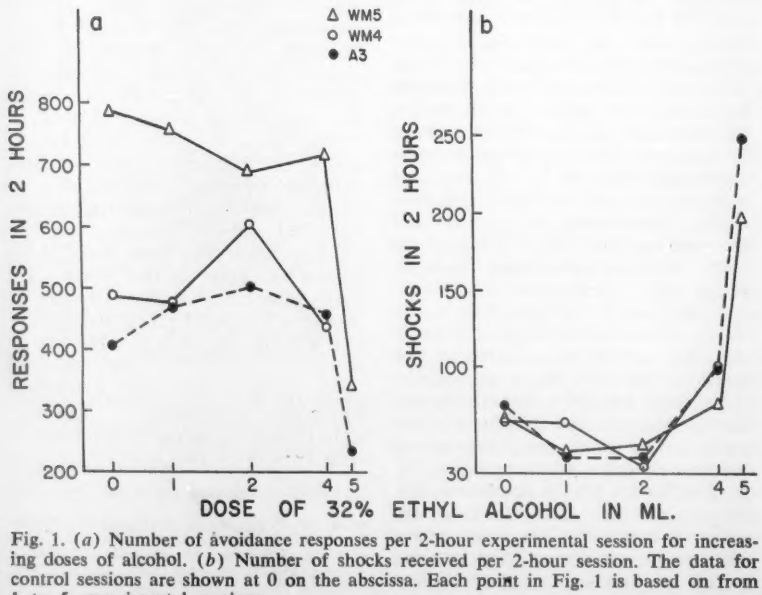

Fig. 1. (*a*) Number of avoidance responses per 2-hour experimental session for increasing doses of alcohol. (*b*) Number of shocks received per 2-hour session. The data for control sessions are shown at 0 on the abscissa. Each point in Fig. 1 is based on from 1 to 5 experimental sessions.

The periods of no responding are correlated with the onset of alcohol-induced paralysis, which has a caudorostral progression in the skeletal musculature. The avoidance response consisted in moving in a circle away from the lever on the front legs, rising on the hind legs, and falling on the lever with the front paws to complete the circle. At the end of the 5-ml session, the hind legs were paralyzed, and only the initial, front-leg portion of the response-series occurred. In both of the animals that received the 5-ml dose, complete skeletal paralysis ensued about 2 hours after injection.

The effect of alcohol upon avoidance behavior may be contrasted with the effects of scopolamine (*2*). Both drugs affect the response rate. In the case of scopolamine this may be owing to a disruption of timing behavior, since the number of shocks received increases in spite of an increase in response rates. With alcohol there is a decrease in shocks received, indicating that the drug is not merely disruptive in its effects at lower dose levels. Similar effects may perhaps be produced by other drugs—for example, barbiturates.

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Reversible Inhibition of Beef Heart Cytochrome c Oxidase by Polyionic Macromolecules

Abstract. The basic proteins protamine (sulfate), histone, lysozyme, and ribonuclease were found to be potent inhibitors of mammalian heart muscle cytochrome *c* oxidase. Their inhibitions were completely reversed in the presence of a strongly anionic polyglucose sulfate. With fresh rat heart muscle homogenates, Keilin and Hartree type of beef heart muscle particulates, and deoxycholate-solubilized oxidase preparations, the reversible nature of the phenomenon was demonstrated with manometric and spectrophotometric assays for cytochrome *c* oxidase.

Subsequent to preliminary observations in this laboratory that cartilage homogenates from certain invertebrate and vertebrate species inhibited mammalian heart muscle cytochrome *c* oxidase, (*1*, *2*), a survey of naturally occurring substances was begun in an attempt to find materials capable of inhibiting or of activating this important terminal respiratory enzyme. At the time this work was started, although unknown to us, Smith and Conrad had reported that protamine, a strongly basic, cationic protein, inhibited the oxidase (*3*). Independent of that report we, also, found protamine sulfate to be a very potent inhibitor of the oxidase, thereby confirming the work of Smith and Conrad. We also found that other basic or cationic proteins, such as histone, ribonuclease, and lysozyme, exert potent inhibitory influences upon heart muscle cytochrome *c* oxidase. Of even greater significance, we believe, is our discovery that all the above inhibitions may be completely reversed by a strongly anionic synthetic polyglucose sulfate. The latter compound was brought to our attention recently when Lash (*4*) and Lash and Whitehouse (*5*) identified and isolated it in extracts of the odontophore cartilage of the marine snail, *Busycon canaliculatum*.

To our knowledge, the present paper constitutes the first report of a reversible inhibition of cytochrome *c* oxidase by polyionic macromolecules of opposite charge.

The following cytochrome *c* oxidase preparations were used: (i) fresh rat heart homogenates, (ii) Keilin- and Hartree-type insoluble beef heart-muscle preparations (*6*), and (iii) deoxycholate-solubilized cytochrome *c* oxidase preparations (*7*), made from the Keilin and Hartree insoluble particulates. Beef heart cytochrome *c* (Sigma) was used in both manometric and spectrophotometric assays of cytochrome *c* oxidase activity, as described previously (*2*). Protamine (salmine) sulfate (General Biochemicals), lysozyme (Sigma, twice recrystallized), calf-thymus histone (Mann), and ribonuclease (Worthington) were used as inhibitors. The polyglucose sulfate corresponds to Mora's "preparation H" (*8*).

In Fig. 1, results of a typical series of manometric oxidase assays are shown. Curve *1* illustrates the oxygen uptake of a control deoxycholate-solubilized oxidase preparation, with hydroquinone as substrate. Curve *2* shows that incorporation of 100 μg of protamine sulfate per milliliter (final concentration) in the above system abolished oxygen uptake completely. In curve *3*, the protamine inhibition was reversed by incorporation of 100 μg of polyglucose sulfate per milliliter (final concentration) into the system. The restored activity in this experiment equalled 80 percent of the original value. In other experiments, depending upon the ratio of protamine sulfate to polyglucose sulfate, complete restoration of activity was readily accomplished.

In Fig. 2, results of a typical set of spectrophotometric oxidase assays are shown. The curves are direct tracings of continuously recorded absorbancy changes made in the Process and Instruments automatic recording spectrophotometer, model RS3. Curve *A*, a control determination, shows a 3-minute decrease in absorbancy (at 550 mμ) of dithionite-reduced cytochrome *c*, as a result of its oxidation by deoxycholate-solubilized cytochrome *c* oxidase preparation. Curve *B* shows the complete inhibition of the oxidase activity of this preparation produced by addition to it of 33 μg of protamine sulfate per milliliter (final concentration). During the first 3 minutes there was no oxidation of the cytochrome *c*, and hence no decrease in absorbancy. Instead, a small increase in absorbancy occurred, as a result of a slight turbidity arising from interaction between the protamine sulfate and the enzyme preparation. This turbidity was responsible for the wavering of the absorbancy values of curve *B*. At the end of 3 minutes of complete oxidase inactivation, readings were briefly interrupted for 20 to 30 seconds (arrow) for the addition of 33 μg of polyglucose sulfate per milliliter (final concentration). In curve *C*

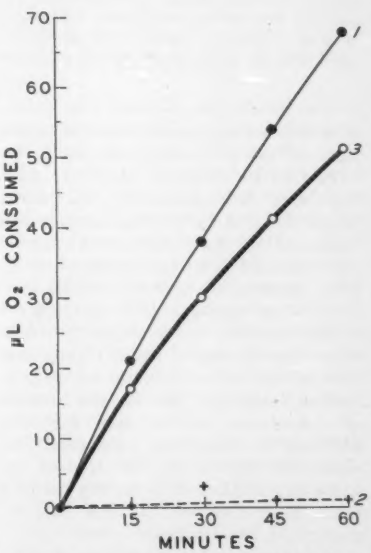

Fig. 1. Manometric demonstration of inhibition of beef heart muscle cytochrome *c* oxidase by protamine sulfate and reversal of protamine inhibition by polyglucose sulfate. Oxygen consumption versus time with hydroquinone as substrate: *T*, 37°C; gas phase, air; final volume, 3.0 ml. All values corrected for auto-oxidation of substrate. Curve *1* (control), O_2 uptake of untreated deoxycholate-solubilized oxidase preparation. Curve *2*, complete inhibition of oxidase by incorporation of protamine sulfate (final concentration 100 μg/ml). Curve *3*, reversal of protamine inhibition by incorporation of polyglucose sulfate (final concentration 100 μg/ml) into the system.

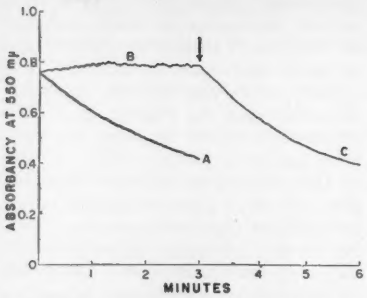

Fig. 2. Spectrophotometric demonstration of inhibition of cytochrome *c* oxidase by protamine sulfate and reversal of protamine inhibition by polyglucose sulfate. Curve *A* (control), oxidation of dithionite-reduced cytochrome *c* by untreated deoxycholate-solubilized oxidase preparation. Curve *B*, complete inhibition of oxidase by incorporation of protamine sulfate (final concentration 33 μg/ml) into the system. Wavering of absorbancy trace was caused by slight turbidity arising from protamine sulfate combination with deoxycholate preparation. Arrow, readings interrupted for 20 to 30 seconds while protamine sulfate (final concentration 33 μg/ml) was added to cuvette with rapid mixing. Curve *C*, reversal of protamine inhibition by polyglucose sulfate addition.

it is shown that not only was the inhibition of the protamine completely reversed by the polyglucose sulfate addition, but, as evidenced by the greater slope of curve *C*, the reaction rate was increased in comparison with that of the untreated control shown in curve *A*. This increase in activity beyond control values resulting from polyglucose sulfate addition is a real phenomenon. It is undoubtedly a result of the fact that in addition to reversal of the protamine inhibition, per se, the amount of polyglucose sulfate used was also sufficient to reactivate some reversibly denatured oxidase in the original enzyme preparation. This finding has also been established by separate heat-denaturation experiments.

Similar reversible inhibitions of cytochrome *c* oxidase have also been demonstrated, using fresh whole rat heart homogenates and the Keilin and Hartree type of insoluble beef heart muscle oxidase preparations. In addition, histone, ribonuclease, and lysozyme were also capable of inhibiting the cytochrome *c* oxidase activity of the above preparations. As with the protamine inhibitions, those produced by the other basic proteins mentioned above could be reversed by polyglucose sulfate.

It is important to stress that the type of reversible inhibition described in this report is in no way specific for cytochrome *c* oxidase. Similar behavior has been established for a number of other enzymes (*9*). However, the fact that cytochrome *c* oxidase may be reversibly inhibited by polyionic macromolecules is of great significance. Until now, the most important reversible inhibitions of cytochrome *c* oxidase have been those accomplished with cyanide, azide, and carbon monoxide. Spectroscopic and spectrophotometric studies of these classic inhibitions have contributed much to our understanding of the enzyme, and in particular, its heme component. It is therefore of great interest that, thus far, spectral changes or shifts during the inhibitions by the cationic macromolecules reported in this paper have not been demonstrable in a large number of experiments. This leads us to suspect that charge-density influences upon the configuration of the protein (and other nonheme components) of the oxidase aggregate may be operating in the present type of inhibitory action. Effects that involve electron transfer through the nonheme moieties of the enzyme are now under consideration.

The disclosure that substances such as protamine, histone, and ribonuclease, which are important intracellular proteins, can exert a reversible inhibition of a major enzymatic component of cell mitochondria also has important implications for the study of nuclear-cytoplasmic metabolic interrelations. Finally, since spectrophotometric evidence, to date, indicates that the heme component of the oxidase is not involved in these inhibitions, we believe that a means is now available for approaching the study of the contributions of the nonheme components of cytochrome *c* oxidase to the electron transfers accomplished by the enzyme.

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8 March 1960

Trophic Substances in a Blind Cave Crayfish

Abstract. The eyestalks, supraesophageal ganglia, and circumesophageal connectives of the blind cave crayfish *Orconectes pellucidus australis* contain a red pigment-concentrating substance and a distal retinal pigment light-adapting one. Assays were performed on the dwarf crayfish, *Cambarellus shufeldti*. The significance of these findings is discussed in relation to endocrine regulation of pigmentary effectors in crayfishes.

The crayfishes *Cambarellus shufeldti* and *Orconectes clypeatus* produce some substances that concentrate chromatophoral pigments and others that cause the distal retinal pigment to migrate toward the fully light-adapted position (*1–4*). No one has determined whether such trophic substances occur in cave crayfishes which lack chromatophores and retinal pigment cells.

Recently we were fortunate to obtain enough specimens of *Orconectes pellucidus australis* (*5*) to learn whether this organism produces a red pigment-concentrating hormone and a distal retinal pigment light-adapting substance. These crayfish were collected in Shelta Cave, Huntsville, Alabama. The specimens of *Cambarellus shufeldti* used as assay

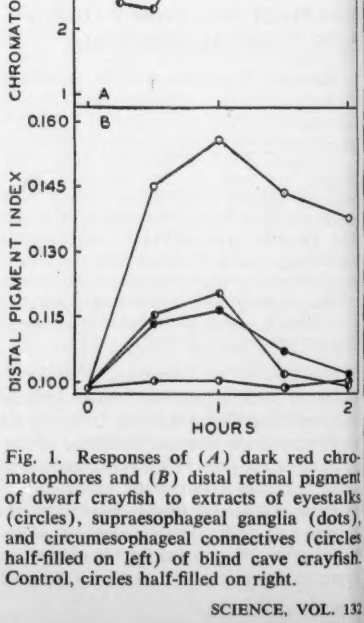

Fig. 1. Responses of (*A*) dark red chromatophores and (*B*) distal retinal pigment of dwarf crayfish to extracts of eyestalks (circles), supraesophageal ganglia (dots), and circumesophageal connectives (circles half-filled on left) of blind cave crayfish. Control, circles half-filled on right.

animals were collected in roadside ditches at Hickory, Louisiana.

The method of Sandeen and Brown (*6*), as modified by Fingerman (*3*) for use with crayfishes, was employed to determine the effect of an extract on the distal retinal pigment of *Cambarellus*. Crayfish were placed one at a time, ventral surface down, on the stage of a stereoscopic dissecting microscope. With the aid of an ocular micrometer and transmitted light two measurements were made: (i) the width of the translucent distal portion of the compound eye in a plane parallel to the long axis of the eyestalk and (ii) the length of the eye from the corneal surface to the apex of the notch at the proximal portion of the eye. The ratio of length of clear area (measurement i) to total length (measurement ii) is known as the distal pigment index.

The dark red chromatophores in the portion of the carapace of *Cambarellus* dorsal to the heart were staged according to the system of Hogben and Slome (*7*). Stage 1 represents maximal concentration of pigment; stage 5 maximal dispersion; and stages 2, 3, and 4 the intermediate conditions.

Student's *t*-test was used for determination of the level of significance between means. The 5 percent probability level was considered the maximum for a significant difference. The results of the statistical analyses comprise Table 1.

Eyestalks, supraesophageal ganglia, and circumesophageal connectives of three crayfish were each extracted in 0.6 ml Van Harreveld's solution (*8*). The extracts were assayed for red pigment-concentrating hormone and distal retinal pigment light-adapting hormone on six dwarf crayfish in black containers maintained under an illumination of 29 ft-ca. Each assay animal received 0.02 ml of extract. Each control animal received 0.02 ml of Van Harreveld's solution. The second time the experiment was performed each extract was injected into seven animals. The results of the two experiments were essentially the same and were averaged (Fig. 1).

Chromatophore stages, determined 15 and 30 minutes after the extracts were administered, were subjected to statistical analysis. The amount of red pigment concentration produced by each extract was statistically significant (analyses 1–3). Furthermore, the response to the eyestalk extracts was significantly less than the response to extracts of either the supraesophageal ganglia or circumesophageal connectives; the response to the supraesophageal ganglia was in turn significantly less than the response to the circumesophageal connectives (analyses 4–6).

Table 1. Summary of the statistical analyses. *N* is number of chromatophore indices or distal pigment indices used in the analysis; S.D. is standard deviation; S.E. is standard error of the difference between the means; *t* is Student's *t*; *p* is probability value. The letters signify extracts of: (C) control, (CC) circumesophageal connectives, (ES) eyestalks, (SG) supraesophageal ganglia.

Analysis	Extract	*N*	Mean	Range	S.D.	S.E.	*t*	*p*
			Red pigment concentration					
1	ES	23	4.00	2.0 –5.0	0.724	0.157	5.75	0.001
	C	24	4.97	4.0 –5.0	.204			
2	SG	24	2.91	1.0 –4.0	.865	.181	10.71	.001
	C	24	4.97	4.0 –5.0	.204			
3	CC	24	2.37	1.0 –4.0	.672	.144	13.98	.001
	C	24	4.97	4.0 –5.0	.204			
4	ES	23	4.00	2.0 –5.0	.724	.232	4.54	.001
	SG	24	2.91	1.0 –4.0	.865			
5	ES	23	4.00	2.0 –5.0	.724	.204	6.84	.001
	CC	24	2.37	1.0 –4.0	.672			
6	SG	24	2.91	1.0 –4.0	.865	.224	2.18	.05
	CC	24	2.37	1.0 –4.0	.672			
			Light adaptation of distal pigment					
7	ES	22	0.150	0.10–0.19	0.0250	0.00604	12.83	0.001
	C	26	.100	.08– .13	.0118			
8	SG	26	.114	.09– .13	.0115	.00322	3.89	.001
	C	26	.100	.08– .13	.0118			
9	CC	26	.117	.08– .16	.0197	.00451	3.63	.001
	C	26	.100	.08– .13	.0118			
10	ES	22	.150	.10– .18	.0250	.00603	9.18	.001
	SG	26	.114	.09– .13	.0115			
11	ES	22	.150	.10– .18	.0250	.00680	6.22	.001
	CC	26	.117	.08– .16	.0197			
12	SG	26	.114	.09– .13	.0115	.00448	0.637	>0.5
	CC	26	.117	.08– .16	.0197			

Distal pigment indices obtained 30 and 60 minutes after the extracts had been injected were also subjected to statistical analysis. The amount of light-adaptation produced by each extract was statistically significant (analyses 7–9). The degree of light-adaptation produced by the eyestalks was greater than that produced by either the supraesophageal ganglia or the circumesophageal connectives (analyses 10 and 11), but no significant difference was found between the responses to the extracts of the supraesophageal ganglia and the circumesophageal connectives (analysis 12).

Fingerman (*1*, *2*) found that the circumesophageal connectives of a specimen of *Cambarellus shufeldti* or *Orconectes clypeatus* contain more red pigment-concentrating hormone than do the supraesophageal ganglia, which in turn contain more of the hormone than is present in a pair of eyestalks. The same order was found for organs of *Orconectes pellucidus australis* (Fig. 1*A*).

Fingerman, Mobberly, and Sundararaj (*4*) found that the supraesophageal ganglia of *Cambarellus* produce more light-adaptation of the distal retinal pigment than do the eyestalks. In contrast, eyestalks of *Orconectes clypeatus* contain more light-adapting hormone than the supraesophageal ganglia (*4*) just as was observed with organs of *Orconectes pellucidus australis* (Fig. 1*B*).

The cave crayfish used in this investigation possesses neither retinal pigments nor chromatophores. Presumably, this cave dweller descended from eyed forms that possessed chromatophores and retinal pigments as well as mechanisms to regulate migration of their chromatophoral and retinal pigments. The persistence of activators of these pigments in blind specimens indicates that either the loss of the controlling mechanism takes longer than the loss of the end organ, or blind forms have given these trophic substances a new function (*9*).

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9 March 1960

Letters

Radiation Hazards

It is reported in "Science in the News" (*1*) that fallout amounts to about 1 percent of the man-made radiation. Although this evaluation is given in somewhat ambiguous terms, fallout radioactivity is placed in the same category as the radiation hazards from wrist watches and TV sets.

The most recent data derivable from reports (*2*) issued by the Atomic Energy Commission and its laboratories allow an accurate evaluation of the fallout radioactivity for the specific time period of March 1959 through February 1960. The total contribution of Zr^{95}, Ru^{103}, Ru^{106}, Cs^{137}, Ce^{141}, and Ce^{144} amounts to 68 millirad, if a uniformly contaminated, infinite, smooth plane is assumed. These data were obtained for land in the Chicago area where the natural background radiation, including cosmic rays, is 97 mrad/yr. (*3*).

Thus, for the most recent period for which data are available the "open field" radiation level averaged 67 percent of natural background radiation. It is to be expected that the fallout activity will decrease markedly during 1960, provided nuclear tests are not resumed. For example, the fallout level for January of this year averaged about 25 percent of the background radiation.

The highest value measured for fallout in the United States, exclusive of local "hot spots," was recorded during April 1959 as 8.41 μrad/hr, or a full 75 percent of that from natural sources (*4*). The principal contributor to the fallout dosage at that time was 65-35 day Zr^{95}-Nb^{95}, which accounted for 78 percent of the total. This relatively short-lived activity gained prominence in fallout due to the unexpectedly fast global deposition of fission products from the Soviet series of tests in October 1958. Charles Dunham, director of the Division of Biology and Medicine of the Atomic Energy Commission, has stated (*5*) that a report on "hot spots" and short-lived activities in fallout will be issued soon.

A comparison has been made at Argonne National Laboratory between the calculated radiation dose from fallout and radiation as measured by a sensitive ionization chamber (*6*). For the April 1959 period, a measured value of 18.5 μrad/hr compares with a calculated value of 19.5 μrad/hr for natural background radiation plus fallout.

The "open field" radiation levels may be criticized on the basis that they do not apply to real radiation doses absorbed by human beings, since people spend much of their time inside buildings where physical factors such as geometry and absorption serve to reduce the radiation dose. This shielding effect is difficult to estimate, being different for rural and metropolitan structures. One would expect, however, that an average shielding factor of 4 might apply.

Spokesmen for the Atomic Energy Commission, the Public Health Service, and the Federal Radiation Council (*7*) have been somewhat ambiguous in statements made about fallout. In giving values for fallout radiation levels they sometimes fail to specify what fallout nuclides are assumed to be involved, what time period is covered, and how the radiation dose is meant to apply.

The situation is even more complex with regard to the reporting of the internal hazard associated with the uptake of fission debris in human beings. The Fallout Prediction Panel convened by the Joint Committee on Atomic Energy predicted, in the course of an investigation by the Committee in May 1959 (*8*, p. 1793) that, in the latitude zone 20° to 60°N, there would be an 8 strontium unit "average Sr^{90} equilibrium bone level corresponding to average maximum deposition from weapons tests to date." The time of maximum retention of Sr^{90} is still a number of years in the future, and one must be careful to take this into account in reporting on present levels of Sr^{90} in human beings. Additionally, one should be careful to present the data for pertinent age groups and not average in adults, for whom Sr^{90} uptake is small. There is also the problem of estimating how many individuals will exhibit a higher uptake of Sr^{90} than the average of 8 strontium units predicted for the North Temperate Zone. Jack Schubert has estimated (*8*, p. 1638) that Sr^{90} displays a log-normal distribution in human beings and that 28 percent of a sampled group will retain three or more times the average (geometric mean) bone burden of Sr^{90}. I have stated (*9*) that a significant number of the young population will accumulate a Sr^{90} burden delivering a lifetime radiation dose to the bone comparable to that from all natural sources of penetrating radiation.

If these data are accepted, then both the external and internal hazards associated with radioactive fallout cannot be placed in the 1 percent category.

RALPH E. LAPP

1315 Park Terrace Drive,
Alexandria, Virginia

References and Notes

1. *Science* **131**, 1656 (3 June 1960).
2. "Quarterly Statement on Fallout," *U.S. Atomic Energy Comm. Rept. No. 4* (Apr. 1960); P. F. Gustafson, "Calculation of Background Gamma-ray Dose Based on Measurement of Radioactivity in Soil" (undated memorandum).
3. See P. F. Gustafson, L. D. Marinelli, S. S. Brar, *Science* **127**, 1240 (1958).
4. P. F. Gustafson and L. D. Marinelli, "Fallout Radioactivity and the Dose Arising Therefrom at Argonne National Laboratory, Lemont, Ill.," appendix to the statement of A. M. Brues in "Fallout from Nuclear Weapons Tests," *Proc. Joint Committee on Atomic Energy* (1959), vol. 2, p. 1400.
5. C. Dunham, verbal comment made during the Joint Committee on Atomic Energy hearings on radiation protection criteria and standards (31 May 1960).
6. P. F. Gustafson, "Assessment of the Radiation Dose Due to Fallout," *Argonne Natl. Lab. Mem.* (1 Oct. 1959).
7. "Radiological Health Data," *U.S. Public Serv. Rept. No. PB 161371-1* (Apr. 1960); "Radiation Protection Standards," Federal Radiation Council staff report (13 May 1960).
8. "Fallout from Nuclear Weapons Tests," *Proc. Joint Committee on Atomic Energy* (1959), vol. 2.
9. R. E. Lapp, *Bull. Atomic Scientists* **15**, 311 (1959).

Our news article was only a summary of what appeared to be the commonly accepted view among the scientists testifying at the radiation hazard hearings. Our wording, as Ralph Lapp points out, was ambiguous. In particular, we should have made it clear that the reported figures referred to the accumulated dose over a period of years. At this time, while fallout is at its peak, it is, as Lapp points out, substantially more than 1 percent of background radiation, although this does not necessarily conflict with the view that the accumulated dose over, say, a 30-year period will be roughly equal to that from television sets or luminous watch dials.—ED.

Education and Research

As an interested outsider to the academic field, I have been keenly following the minor debate on teaching and research. May such an outsider offer an opinion?

The question put in the editorial [*Science* **131**, 71 (8 Jan. 1960)], "why . . . should some instructors oppose the recognition of good research as a consideration second to good teaching?" is, it seems to me, answered by the spirit displayed in Paul Bohannan's letter [*Science* **131**, 1282 (29 Apr. 1960)]. Bohannan's apparent position, that any scholar not doing research simply cannot be a fully effective teacher, represents the camel whose nose the instructors are trying to keep out of the academic tent by refusing to recognize research at all. Bohannan may be describing a worthy ideal, but F. J. Allen's letter [*Science* **131**, 944 (25 Mar. 1960)] has the honest ring of reality.

Surely, a well-balanced view of the situation would run something like this:

1) The primary mission of a college is to educate its students, not to conduct research.

2) Research at such an institution is desirable for two reasons: (i) for the educative value of exposing the student to an environment in which research is

conducted; (ii) to attract instructors who, by reason of inclination and ability, wish to divide their time between research and teaching.

3) It is not necessary under these circumstances that all instructors be researchers.

4) Due recognition being given to the contribution that research makes to education, all faculty members should be evaluated on the basis of their contributions, made in various ways, to the primary mission: education.

My own undergraduate experience of a decade ago tends to bear out Allen's contention; the better teachers on that level were not deeply involved in research. At any rate, an attempt to find the proper place for research in a college is not at all helped by broad claims for the essentiality of research.

SANDER RUBIN
60 South Munn Avenue,
East Orange, New Jersey

Differentiation of Basaltic Rock

In his excellent article on granite, Walton [*Science* **131**, 635 (1960)] discussed the problem of deriving granitic rock from a parent basaltic magma. He summarized the classic ideas on processes of magmatic differentiation, and he elaborated on mechanisms for selective mobilization of certain rock-forming components under high temperatures and pressures. As he noted, the components at the apex of Bowen's reaction series will be selectively mobilized in the presence of high pressure of water vapor and temperature of about 700°C. This is the means by which granitic rock tends to be formed at the expense of other rocks. His statement, however, neglected a consideration of differentiation at the surface of the earth; and yet this is a fundamental part of the geologic cycle.

Surficial processes of weathering, erosion, transportation, and deposition accomplish a differentiation of rock-forming components. Some of the products of weathering of, for example, basaltic rock, are carried in solution or colloidal suspension, and others are carried as detrital particles. The resulting sediments include cherts, iron oxides, carbonate rocks, evaporites, and detrital sediments, with hydrated clays and connate water. There may be a geographic concentration of some of these deposits in some places and others in other places.

Nonetheless, the total complex of materials that accumulate in a geosyncline should approximate the total complex of materials eroded from the source region. But, the rock-forming components are arranged differently in the sediments than in the source rocks; and with geosynclinal downwarping to zones of higher temperature and pressure, the now-unstable components in the sedimentary rocks will react more readily to selective mobilization than will the components in basaltic rock. Thus, in one or more geologic cycles, granitic rock can be derived from basaltic rock, through surficial processes followed by plutonic processes.

BREWSTER BALDWIN
Department of Geology and Geography,
Middlebury College,
Middlebury, Vermont

I welcome Baldwin's commentary, which adds another perspective to my review of "Granite problems." He and I studied together under that staunch magmatist S. J. Shand, who was wont to remark that sediments bear the same relationship to rock as sawdust does to the living tree. Shand said it with an ironic twinkle in his eye, and yet there were overtones of the traditional "hard-rock" school, which tended to regard weathering and sedimentation as the terminus of the rock-forming process rather than a stage in a major geochemical cycle. If, in not dealing explicitly with this broader aspect of the problem, I betrayed relics of a "hard-rock" bias, I am glad it is an old Shand man and fellow student who puts the matter straight.

MATT WALTON
Department of Geology, Yale University, New Haven, Connecticut

Evaluating New Drugs

I would like to point out one error in the article on drug hearings [*Science* **131**, 1299 (29 Apr. 1960)]. It is stated that at the present time physicians have no convenient index for evaluating pharmaceutical products except for the printed information from the various drug manufacturers.

In 1959 the bi-weekly *Medical Letter* began publication. This is a publication of Drug and Therapeutic Information, Inc., 136 E. 57 St., New York 22. The *Medical Letter* is a nonprofit publication having as its aim the dissemination to the medical profession of information concerning manufactured drugs. It has an editorial board of university faculty members who advise, through the medium of this publication, what is a valuable addition to the therapeutic armamentarium and what is not. The board of editors also frequently points out differences in the costs of similar products, comparing the prices of drugs under generic and trade names. This publication is available on subscription.

GRAHAM A. VANCE
165 Green Bay Road,
Wilmette, Illinois

Meetings

Southwestern and Rocky Mountain Division

The Southwestern and Rocky Mountain Division of the American Association for the Advancement of Science held its 36th annual meeting in Alpine, Tex., 1–5 May 1960.

Members of the division were special guests of Sul Ross State College at ceremonies for the dedication of the new Science Building on 2 May. Later that day Chauncey D. Leake, president of the AAAS, delivered the opening address of the meetings, speaking on "Communications among scientists in relation to the unity of science." Dael Wolfle reported on the general activities of the Association.

Programs of the sections of the division included 48 individual papers. Two symposia consisting of invited papers were conducted. One of these, extending through two sessions, was sponsored by the division's Committee on Desert and Arid Zones Research. It was presented by eight specialists in the fields of agriculture and forestry and dealt with problems of water yield in the Southwestern United States. In the other, the fifth in a series of symposia on the improvement of science teaching, curriculum studies in the fields of science were discussed. Members of each of the curriculum study groups were present to conduct these discussions.

The division's annual John Wesley Powell memorial lecture was presented by Knox Taylor Millsaps, chief scientist, Air Force Missile Development Center, Holloman Air Force Base, who spoke on fluid flow in circular pipes.

Retiring divisional president Lora M. Shields, professor of biology of the New Mexico Highlands University, delivered the presidential address, entitled "No life for a lady."

Members in attendance were special guests of the McDonald Observatory, on Mount Locke, on one evening during the meetings. The observatory staff very graciously set aside their regular observation program to demonstrate the 82-inch refractor telescope and to give the visitors a view of the heavens such as few of them had ever seen before.

The final day of the meeting was devoted to a field trip into the Big Bend National Park. Under the direction of park naturalist Harold Broderick, the group was able to observe many of the interesting geological features of the area, and with the desert flora in full show, the botanical observations were equally interesting.

Newly elected officers of the division include Alan T. Wager (Arizona State University), president; Anton Berk-

man (Texas Western College) president-elect; and Harold A. Dregne (New Mexico State University), member of the executive committee. Marlowe G. Anderson (New Mexico State University) will continue as secretary-treasurer and council representative.

MARLOWE G. ANDERSON
New Mexico State University, University Park

Forthcoming Events

July

24–19. Modern Physical Theories and Associated Mathematical Developments, Boulder, Colo. (K. O. Friedricks, New York Univ., 25 Waverly Pl., New York)

25–6. International Assoc. of Physical Oceanography, 13th general assembly, Helsinki, Finland. (B. Kullenberg, c/o Oceanografiska Institutet, P.O. Box 1038, Goteborg 4, Sweden)

26–28. Poliomyelitis, 5th intern, conf., Copenhagen, Denmark. (S. E. Henwood, International Poliomyelitis Congress, 120 Broadway, New York 5)

27–12. Mathematical Statistics and Probability, symp., Berkeley, Calif. (A. P. Burroughs, Air Force Office of Scientific Research, Research Information Office, AFOSR/USAF, Washington 25)

28–29. Computers and Data Processing, 7th annual symp., Estes Park, Colo. (W. H. Eichelberger, Denver Research Inst. Univ. of Denver, Denver 10, Colo.)

30–6. Institute on Religion in an Age of Science, 7th annual conf., Star Island, N.H. (R. Burhoe, American Acad. of Arts and Sciences, 280 Newton St., Brookline 46, Mass.)

31–5. Alcohol and Alcoholism, 26th intern. cong., Stockholm, Sweden. (A. Tongue, Bureau International contre l'Alcoolisme, Case Gare 49, Lausanne, Switzerland)

31–5. Photobiology, 3rd intern. cong., Copenhagen, Denmark. (A. Hollaender, Biology Div., Oak Ridge Natl. Laboratory, Oak Ridge, Tenn.)

31–6. Psychology, 16th intern. cong., Cologne, Germany. (Prof. Undeutsch, Psychology Inst. Universität, Cologne)

31–7. Anthropological and Ethnological Sciences, 6th intern. cong., Paris, France. (H. Vallis, Directeur, Musée de l'Homme, Palais de Chaillot, Place du Trocadéro, Paris 16°)

August

1–3. Global Communications, 4th symp., Washington, D.C. (R. L. Clark, c/o Office of Director of Defense Research and Engineering, Washington 25)

1–6. Esperanto Cong., 45th annual intern., Brussels, Belgium. (45-a Universala Kongreso de Esperanto, Brussels)

1–12. Modulation Theory and Systems, Cambridge, Mass. (E. J. Baghdady, Dept. of Electrical Engineering, Massachusetts Inst. of Technology, Cambridge)

2–5. Poultry Science Assoc., Davis, Calif. (C. B. Ryan, PSA, Dept. of Poultry Husbandry, Texas A & M College, College Station)

3–6. Gas Chromatography (Infrared Spectroscopy Inst.), Nashville, Tenn. (N. Fuson, Fisk Infrared Inst., Fisk Univ., Nashville 8)

3–6. Rarefied Gas Dynamics, 2nd intern. symp. (by invitation only), Berkeley, Calif. (Engineering and Science Extension, Univ. of California, 2451 Bancroft Way, Berkeley 4)

5–6. Pennsylvania Acad. of Science, summer annual, Grantham, Pa. (K. B. Hoover, Messiah College, Grantham)

6–12. International Geographical Cong., 19th, Stockholm, Sweden. (IGC, Postfach, Stockholm 6)

7–10. American Soc. of Clinical Hypnosis, Miami, Fla. (S. Hershman, 6770 N. Lincoln Ave., Chicago 46, Ill.)

7–12. Gerontology, 5th intern. cong., San Francisco, Calif. (L. Kuplan, Intern. Cong. of Gerontology, P.O. Box 2103, Sacramento 10, Calif.)

7–13. Industrial Research Conf., Harriman, N.Y. (Miss M. F. Garvey, Industrial and Management Engineering Dept., Columbia Univ., New York 27)

8–11. American Astronautical Soc., Seattle, Wash. (R. M. Bridgforth, AAS, Propulsion Unit, Boeing Airplane Co., Aero-Space Div., P.O. Box 3707, Seattle)

8–12. American Inst. of Electrical Engineers, San Diego, Calif. (R. S. Gardner, AIEE, 33 W. 39 St., New York 18)

8–13. World Federation for Mental Health, 13th annual, Edinburgh, Scotland. (Secretariat, WFMH, 19 Manchester St., London, W.1, England)

8–20. American Soc. of Criminology, London, England. (D. E. J. MacNamara, New York Inst. of Criminology, 115–117 W. 42 St., New York 36)

9–13. Hail Storms, intern conf., Verona, Italy. (H. G. M. Ligpa, American Meteorological Soc., Stanford Research Inst., Stanford, Calif.)

11–13. Rocky Mountain Radiological Soc., Denver, Colo. (J. H. Freed, 4200 E. Ninth Ave., Denver 20)

11–16. Canadian Teachers Federation, Winnipeg, Manitoba. (G. G. Croskery, 444 MacLaren St., Ottawa 4, Ontario)

14–19. American Pharmaceutical Assoc., Washington, D.C. (R. P. Fischelis, APA, 2215 Constitution Ave., NW, Washington 7)

14–19. International Cong. of Clinical Chemistry, Edinburgh, Scotland. (S. C. Frazer, Clinical Laboratory, Royal Infirmary, Edinburgh)

14–20. Cardiology, 6th Inter-American cong., Rio de Janeiro, Brazil. (H. Alqueres, P.O. Box 1594, Rio de Janeiro)

15–16. National Assoc. of Boards of Pharmacy, Washington, D.C. (P. H. Costello, 77 W. Washington St., Chicago, Ill.)

15–17. Heat Transfer Conf., ASME and AICE, Buffalo, N.Y. (A. B. Conlin, Jr., ASME, 29 W. 39 St., New York 18)

15–17. Organic Scintillation Detectors, intern. conf., Albuquerque, N.M. (G. H. Daub, Chemistry Dept., Univ. of New Mexico, Albuquerque)

15–18. American Veterinary Medicine Assoc., Denver, Colo. (H. E. Kingman, Jr., 600 S. Michigan Ave., Chicago 5)

15–18. Radiation Biology, 3rd Australian conf., Sydney, Australia. (P. Ilbery, Dept. of Preventive Medicine, Univ. of Sydney, New South Wales, Australia)

(See issue of 17 June for comprehensive list)

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Virologist and Tissue Culturist to practice in tumor virus program. Please state experience and salary desired. Box 142, SCIENCE. 7/1

SYMPOSIUM

The Fifth International Symposium on Free Radicals

will be held at Uppsala University, Sweden, on July 6 and 7, 1961.

Emphasis will be on experimental methods for studying properties of free radicals and interactions involving trapped radicals in condensed systems at low temperatures.

Those wishing to receive the First Circular, in September 1960, about the meeting should write to: Fifth International Symposium on Free Radicals, Institute of Physical Chemistry, Uppsala, Sweden.

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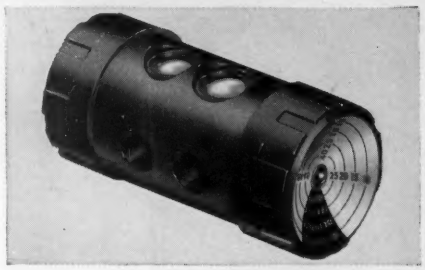

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